

THE Psychological Review

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THE PSYCHOLOGICAL REVIEW.

THE CLASSIFICATION OF PSYCHO-PHYSIC METHODS.

By DR. EDWIN B. HOLT.

Every one who has undertaken experiments on the relation of stimulus to sensation, or even read considerably on the subject, must have become aware of the inadequacy of the historical and still current classification and designation of the psycho-physic methods. For many cases arise in practice which have no immediate place in the classification, as for instance those in which it is necessary to recognize and more especially to evaluate judgments other than those of 'less,' 'equal' and 'greater' (as say 'much less' 'uncertain' and 'much greater'); and other cases arise whose place is doubtful because they have features of several of the methods but have not all the essentials of any one of them.

If for instance one were to wish to find the acuteness of a given sense in an interesting pathological patient, and if from independent reasons (as may well happen) one were obliged to adopt the so-called method of right and wrong cases, one might be brought to pause, if the subject were impatient or perhaps hysterical, by the appearance of adverse emotions and fatigue, due to the almost endless repetition of just two stimuli which this method requires. And yet perhaps one could get from the subject one fifth the required number of judgments on each of five different pairs of stimuli, or else one tenth of the number from ten pairs. Now clearly if one were careful to have one stimulus common to these five or ten pairs, one could calculate by the method of least squares from the five or ten resulting

groups of judgments the coefficient of precision ('*Präzisionsmass*') with as much accuracy as otherwise from one group of ten times as many judgments, on a single pair of stimuli. Yet in such an instance one would not be using the method of right and wrong cases. For although the point of departure and the answer yielded conform to the descriptions of this method, yet the actual data gotten and used in calculating the result are precisely such as one would have gotten from the method of least differences, or minimal changes (*Abstufungsmethode der kleinsten Unterschiede*) if peculiarities of the sense-organ investigated or exigencies of apparatus available had forced one to use few and coarse gradations of stimuli and to present the different

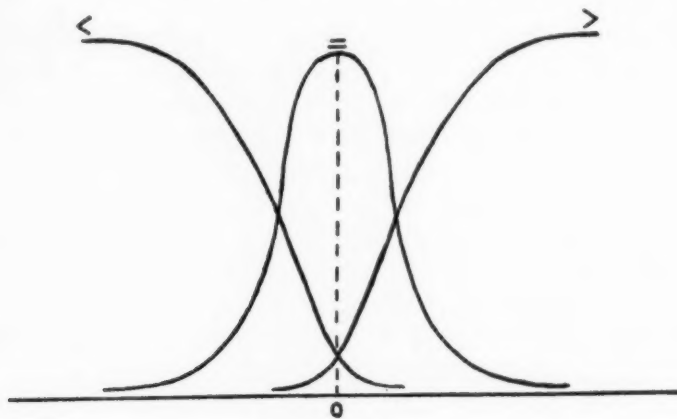


FIG. 1.

pairs at random. For in both cases the data would consist in judgments of 'less' (<), 'equal' (=) and 'greater' (>), which would be distributed as shown in Fig. 1, where the abscissæ represent the value of the stimuli greater or less than the standard stimulus (0) which is common to the ten pairs, and where the ordinates represent the number of judgments.

In short the experiment described does not come under any one of the traditional methods, since it has also features of at least one other method, nor can it properly be called a 'combination' of the methods, since it has not all the essential features of any one.

Apart from the confusion which every reader or experimenter in psychology has probably felt in assigning a given experiment to one of the four or six 'methods,' the unsatisfactory state of the whole matter is amply witnessed by the historic errors made by the most distinguished experimenters in designating the methods they have used, by the confused and overlapping definitions of the several methods invariably given in text-books, and by the amendments offered from time to time by various theorists. The errors of designation, although several were made by Fechner himself¹ are chiefly of historic interest. Of systematic interest, however, are the current definitions and descriptions of the psycho-physic methods, and the several emendations which their inconsistencies have elicited.

The case above cited of an experiment which, by having features of two methods and yet lacking the essentials of any one, belongs properly under none of the four or six methods, is not wholly unprovided for in the treatises. Thus Wundt,² in defining the method of mean gradations, describes, for the case that the middle stimulus varies irregularly, a certain simple way of deriving the mean value from the raw data: but this way need not be used, he says, since the raw data can also be treated by the method of right and wrong cases, in which case the whole procedure becomes 'a combination of the method of mean gradations with the method of right and wrong cases.' Now the simpler way of evaluating the data applies exclusively to the method of mean gradations: is it then essential to this method? Clearly not, since it may be abandoned in favor of the method of right and wrong cases. But then the method of mean gradations is not, as is generally supposed, a method for both getting and then evaluating raw data, but only for getting them. Conversely, too, the method of right and wrong cases is not at all a method for getting data but only for treating them when gotten. But if this is true, these two methods are not alternatives to be chosen between, but supplementaries to be used in combination; a conclusion which is at variance with the present theory and practice.

¹ Müller, G. E.; *Zur Grundlegung der Psychophysik*, Berlin, 1879, S. 56 ff.

² Wundt, W.; *Grundz. d. physiol. Psych.*, 5te Aufl., Leipzig, 1902, I Bd., S. 480.

This contradiction arises, of course, from the confused definitions of the methods, whereby these latter, instead of being four mutually-exclusive logical classes, as they should be, are defined as being now exclusive, now partially identical, now coördinate, and now not coördinate. And this confusion extends through the whole tissue of the methodology, at least the Wundtian. For this author, after dividing all methods in two (mutually-exclusive ?) groups, of gradation (*Abstufung*) and of telling-off (*Abzählung*), explains¹ that 'Among the telling-off methods the method of mean errors is most nearly related in its origin to the gradation methods or more particularly to the method of minimal changes.' Indeed,² 'The method of mean errors arises from the method of minimal changes in case one limits oneself to taking the just not-perceptible differences of stimuli.' By this is presumably meant that the raw data in the method of mean errors are identical with a part of the data in the method of mean gradations; and the fact is left out of account that the data are further quite differently treated in the two cases, in ways which by no means 'arise' the one from the other. In short Wundt's systematic treatment of psycho-physic method stops short of logically exact and consistent definition.

Yet these ambiguities are by no means peculiar to Wundt; firstly because most other modern treatises follow, or perhaps even copy, the veteran psychologist, secondly because the same ambiguities have prevailed from the very first. Thus while Wundt finds the method of minimal changes merging into that of mean errors, Fechner³ found that it 'goes over' into the method of right and wrong cases! The key to the situation is this: the four psycho-physic methods are historical developments bearing the marks of their growth and of the accidents they have met. The methods have not been defined, they have been *used*; and where a user has had to modify a method he has generally not modified its name, so that there are several varieties under every method, and these bear the most diverse and unsystematic relations to one another. So great has been

¹ *Ibid.*, S. 473.

² *Ibid.*, S. 472.

³ Fechner, G. T.: *Elemente d. Psychophysik*, Leipzig, 1860, I Th., S. 75.

the dignity of tradition in this matter of method, that a thorough-going revision and consistent systematization of the procedures has never been achieved.

To systematize the procedures is not necessarily to analyze or revise their mathematical details,—an ambitious performance indeed,—but the frame-work of methodology can be reconstructed in and for itself. This frame-work so recast will carry with it undisturbed, as tent-poles the canvas, the vast multitude of details.

To commence, one must first survey the methods in their confusion, then either discover or postulate some one feature as the sole essential of each method, and then deduce the logical consequences. The Wundtian account is the most characteristic and may well be made the starting point.

The methods fall in two groups, those of gradation and those of telling-off. The former group has two classes, the method of minimal changes and that of mean gradations; the latter also two, the method of mean errors and that of right and wrong cases.

I. THE METHOD OF MINIMAL CHANGES.¹

“In this method one seeks to determine at different points on the scale of stimuli such a change in the intensity of stimulus as produces a barely perceptible change of sensation.” And, in detail, one finds what interval between the standard stimulus and a lower, compared stimulus is needed to make the two feel different, and the same between the standard and a higher stimulus; and again the interval is found between standard and lower or higher stimulus which will just not make the two feel different. The average of these four intervals is the threshold wanted.

This is good as a scheme, but on coming to the practice one finds that there is no interval which just is or is not *always* felt as a difference. There are intervals which are generally felt as one or the other, but in order always to be so felt the interval must be either so large or so small that it tells nothing about the accuracy of discrimination which one is trying to find. The scheme appears to be applicable if the compared stimulus can

¹ Wundt, *op. cit.*, SS. 470, 476-479.

vary about the standard *continuously*, for then one commences with the compared stimulus plainly larger, say, than the standard and diminishes the former until it no longer seems larger; one calls this interval the upper threshold of difference, and finds the other three desired intervals similarly. But the continuously applied stimulus tires the sense, while habit retards or expectation hastens (presumably according to the temperament of the subject) the moment in which the relation of the stimuli is felt to change; and it has not been proved that these factors cancel themselves out. Thus the result yielded by such a procedure would be modified by the temperament of the subject and the capacity of his sense-organ to resist exhaustion, and would not be a pure measure of his discrimination.

If the compared stimulus is such as cannot be varied continuously then the procedure in question is, for all careful work, out of the question, for the final result will be found to be not a little dependent on the size of the step-wise gradations which one arbitrarily has adopted in the series of compared stimuli. And furthermore the case will often come up and have somehow to be taken into consideration that one interval will yield a perceptible difference, the next smaller will not, while the next smaller than that will again do so.

In short, whether the compared stimulus is to be varied continuously or step-wise, it is advisable (as Wundt himself admits; S. 478) not to present these stimuli in their orderly progression, but rather in a random succession. But 'this procedure has at the same time the character of a combined method, since it approaches the telling-off methods.' Whether 'combined' or not, it yields three kinds of judgments ('greater' 'equal' and 'less'), and they show the arrangement which we have met before in Fig. 1.

What is now essential to this method of minimal changes? It is not the use of a standard stimulus and compared stimuli, for the method of mean error also uses these; nor is it the 'minimal' gradations in the compared stimulus, for the methods of mean gradations and of mean error require these; nor is it yet the ascertainment of the intervals above and below the standard stimulus which barely do *not* give a difference in sensation, for

the method of mean error involves also this. Essential to the 'method' of minimal changes is only the *project* of finding that difference between stimuli 'which produces a barely perceptible change of sensation.' This, however, is not a method, but a problem: furthermore the quantity so sought needs a precise definition (which the phrase quoted by no means gives) and gets this only from the method finally fixed on for solving the problem. Now the methods of evaluating data described by Wundt as peculiar to the 'method' of mean gradations have been shown to be inadequate for careful work; and in order to be precise, as Wundt himself advises, one must use a method which 'approaches' (to put it plainly *is*) the method of mean error. In a word, the 'method of minimal changes,' is not a method at all, but a problem which one sets oneself. It can be solved with precision only by such a procedure, and by any such, as yields at least three classes of judgments grouped as in Fig. 1. How from these a solution is to be gotten is not told by the 'method of minimal gradations.'

As if so much confusion were not enough, the account provides only for the case of finding that change of stimulus which produces a change of sensation, that is, the threshold of *discrimination*; whereas the case that one wishes to get the threshold of *sensation* may properly come under one of the methods, and certainly can come under no other than this of minimal changes. Finally this threshold of discrimination is an average or two thresholds, those of just perceptible and just imperceptible difference, each of which has often been made the basis of investigation, although in either case the method has generally been called minimal changes. Thus the 'method of minimal changes' not only is a problem and not a method; it is not even a definite, single problem.

2. THE METHOD OF MEAN GRADATIONS.¹

This method, which Wundt substitutes for the older, somewhat more natural, and at least equally important 'method of over perceptible differences,' consists in finding a mean stimulus which shall seem to lie equally far from two chosen extremes.

¹ Wundt, *op. cit.*, SS. 471, 479-481.

Two intervals are to be made equal. 'But in order to get reliable results,' Wundt says, 'it is necessary to combine this method either with that of minimal changes or with one of the two telling-off methods about to be described.' In fact it appears that when the mean, variable stimulus is presented with the two standards, it will seem to lie nearer sometimes the lower, sometimes the higher, standard; sometimes just half-way between. This gives rise to three groups of values for the mean stimulus, those for which it is judged to lie more and less than half-way and just half-way, between the two extreme stimuli. And these three groups are arranged, once more, as in Fig. 1. If only a rough approximation to accuracy is wanted, the informal procedure described under minimal changes can be used in evaluating the data. But as has been shown, this procedure is not permissible if it is a question of careful work. Rather after the three 'groups of values shown in Fig. 1 have been found, these must then be in some so far unexplained way evaluated; in a way, presumably, which will 'approach,' 'resemble' or 'shade off' into one of the telling-off methods about to be described.

In short, mean gradations are no more a method than were minimal changes, but once more a problem. There the problem was to find the threshold of discrimination between sensations; here it is to find the threshold of discrimination between intervals; there the just perceptible difference, here the just equal over-perceptible differences (intervals), between sensations.

Wundt's account of this 'method,' which is doubtless designed to intone the importance of Merkel's 'law,' is peculiar inasmuch as it gives the impression that one must always take the extreme stimuli as fixed and vary the mean stimulus; whereas by far the most work on over-perceptible differences has been done by taking either the upper two or the lower two stimuli as fixed, and the variable stimulus below or above them. When this is done the data still consist in three groups of values for the variable stimulus, according as the variable interval seems greater or less than, or equal to, the standard interval: and these groups still shape themselves as in Fig. 1.

Thus both of the so-called gradation methods are no methods,

but are problems; namely, how to find objective values (measuring the stimuli) which somehow correspond to just perceptible or to equal over-perceptible differences, respectively, between sensations. And either *problem* requires for its solution, firstly, a *procedure* which will yield *three classes of values of the variable stimulus* (corresponding to the judgments 'greater,' 'equal' and 'less') grouped as is shown in a general way by the curves of Fig. 1. Secondly, the problem requires a *procedure* for deriving from these three curves a measure of the discrimination, respectively of difference between sensations or of equality between over-perceptible intervals. The former procedure affords the raw data, the latter evaluates them. In these two processes will be found the real division of psycho-physic methods.

3. TELLING-OFF METHODS: THE METHOD OF MEAN ERROR.¹

This method 'arises from the method of minimal changes in case one limits oneself to taking the just not-perceptible differences of stimuli.' If this were all, then this method would be not an independent method at all, but one of the subdivisions of the so-called method of minimal changes. But it is not all. For while the raw data group once more as in the curves of Fig. 1, we are now for the first time told how to evaluate these data, and to get from the curves a measure of discrimination. The *procedure* becomes here for the first time precise; one finds by definite rules the raw mean error, the variable mean error, the constant mean error; not to mention, as Wundt does not, the probable error, the standard error or deviation, the coefficient of variability, and the relative variability.² The fineness of discrimination is usually taken as the reciprocal of the variable mean error; or, to put it accurately, the discrimination is *defined* as equal to the reciprocal of the variable mean error, called for brevity's sake mean error. By other conventions the other 'errors' are utilized more or less advantageously to give light on the workings of the sense organs under investigation.

¹ Wundt, *op. citat.*, SS. 472-473, 481-482.

² Cf. Yerkes, R. M.: PSYCHOLOGICAL BULLETIN, 1904, Vol. 1, No. 5, p. 137 and Myers, C. S., *Report of the Cambridge Anthropological Expedition*, Vol. II., Pt. II., p. 212.

But this variable mean error measures the sense discrimination not only for the case that one is seeking the just not perceptible difference between stimuli, but also when one wants the just perceptible difference (the quest which is confusingly called the 'method' of minimal changes), or when one wants the just equal over perceptible differences or intervals (called the method of mean gradations). In both these cases also the variable mean error, with the method of finding it, is the precise definition of the terms 'just perceptible difference' and 'equal over-perceptible difference'—terms which were so far loosely used in stating the problems of minimal changes and mean gradations. In short the mathematical manner of evaluating the raw data, which always group as in Fig. 1, is the only exact definition of the measure of discrimination which is being sought.

The method of mean error is then both a problem and a method. Its problem is to find the just not perceptible difference between stimuli. Its method is a mathematical affair of averaging errors and of other details which we are not now concerned with going into; except to note that here at last is a true method. But this method has no intrinsic affiliation with the problem of not perceptible differences: it is equally necessary in evaluating the data on just perceptible differences and just equal over-perceptible differences or intervals. This is a fact which the traditional classification quite obscures, although the actual practice of psychologists will be found to accord with that fact and to belie the tradition.

The three 'methods' of minimal changes, mean gradations, and mean error have amounted so far to *three problems* in three kinds of discrimination, each calling for a special kind of data to be gotten by experiment; and then, *one universally applicable mathematical method* for evaluating these data. The three problems in discrimination may now conveniently be called—just perceptible differences, or *j.p.d.* (from the method of minimal changes), equal intervals that is equal over-perceptible differences, or *e.o.p.d.* (from the method of mean gradations), and not perceptible differences, or *n.p.d.* (from the method of mean error). The one mathematical method for evaluating any of

these data, with its variations, will be called through the rest of this paper the method of mean error, proper. One more *problem* may well be added—that of the threshold of sensation, or *t. of s.* (in comparison with which, as will be recalled, the discriminations just mentioned are often named thresholds of difference).

4. THE METHOD OF RIGHT AND WRONG CASES.¹

This method consists in the repeated presentation to a subject of two stimuli which are so nearly equal that they will frequently be judged to be quite equal, and the greater be sometimes judged even less than the lesser and conversely. Thus the judgments will be sometimes right and sometimes wrong—a fact which every author sapiently points out, as if the same were not true of judgments found under every one of the other

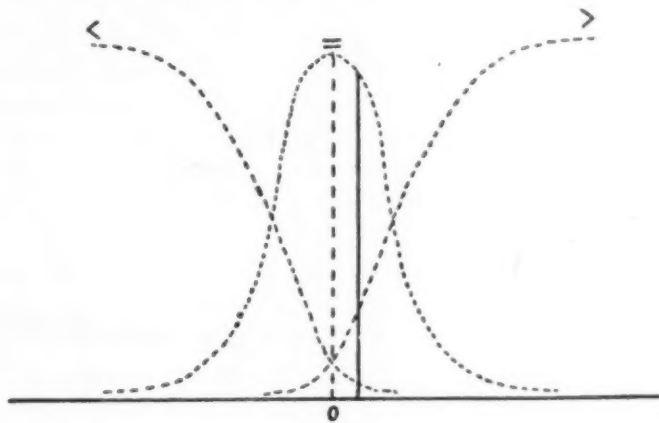


FIG. 2.

'methods.' But the essential fact is that, although these data cannot be grouped like those of Fig. 1, yet they are exactly such a part of Fig. 1 as would lie in an ideally narrow vertical section taken near the zero point of Fig. 1. This is shown in Fig. 2. That is, this method yields as much of the Fig. 1 as a standard stimulus compared with only *one other* stimulus can yield. The judgments gotten are that the compared stimulus

¹ Wundt, *op. cit.*, SS. 473-4, 482-90.

is equal to, greater or less than, the standard; and in order to complete these data into a figure like Fig. 1, it would be necessary only to bring in more compared stimuli, *i. e.*, to use more pairs. This was shown in the second paragraph of this paper. Why sometimes many pair of stimuli are used, and why sometimes only two, does not for the moment concern us.

Now we have seen that the data of Fig. 1 can be evaluated and made to yield a measure of discrimination, by means of a method which we have called the method of mean error. The method of right and wrong cases presents us, in fact, with a *second method* of evaluating data into a measure of discrimination, and relatively fragmentary data at that, *i. e.*, judgments on but two stimuli. It is not the purpose of this paper to discuss these mathematical methods of evaluation in their details; and it is sufficient to recall that a 'Präcisionsmass' is derived from the judgments on two stimuli by means of Gauss's formula for the error curve, or more readily by the use of Fechner's table of integrations derived from that formula. The method assumes that the theory of errors may be applied to the mistakes made in comparing two stimuli, that is, the 'wrong cases'; another fact which is invariably emphasized, although the averages taken in the method of mean error involve exactly the same assumption.

The method of right and wrong cases is seen, therefore, to be no new problem but in fact a new, real method of evaluation, to be used (one is told) when for any reason the stimuli compared have been only two. Right and wrong cases are generally used for finding a 'Präcisionsmass' of the just perceptible difference, but there is no intrinsic reason why the use of two stimuli and Fechner's integral table should not give a measure of equal over-perceptible differences, or even, with the standard stimulus made equal to zero, of the threshold itself.

We have so far analyzed the four so-called psycho-physic methods into four problems (not parallel with the original four 'methods') and two real methods. The problems are those of the *j.p.d.*, *c.o.p.d.*, *n.p.d.*, and that of the threshold of sensation, or *t. of s.* The methods are those of mean error, *m.* of *m.e.*, and right and wrong cases, *m.* of *r.w.c.* Let us

now see what is the relation of these two methods to each other. The *m.* of *m.e.* is commonly recommended; except where the data are derived from the comparison of only two stimuli, in which case the more cumbersome *m.* of *r.w.c.* has to be resorted to. But this is not the real distinction between the two methods. It will be recalled by experimenters that the measure of precision yielded by the *m.* of *r.w.c.* is virtually neither more nor less than the steepness of the curves shown in Fig. 1, or to be more exact, it is a numerical representation of the steepness of the curve of 'greater,' or that of 'less,' judgments (for these two curves are assumed to be ideally similar) when one half of the 'equality' judgments at every point has been added on to this curve (Fig. 3). Now it is always in-

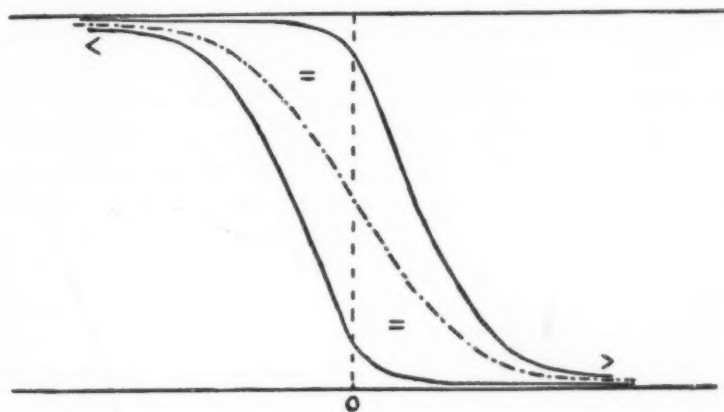


FIG. 3.

sisted that when the judgments are made on but one pair of stimuli, the number taken must be very large indeed if the measure of precision so derived is to be worth anything. But it is seldom if ever suggested, as was done in the second paragraph of this paper, that by the method of least squares the measure of precision may be calculated from more pairs of stimuli and proportionately fewer judgments on each pair. And the results in the two cases will be to all intents and purposes identical. Now this would be applying the *m.* of *r.w.c.* to the full data of Fig. 1: which shows that the distinction between

the methods of *m.e.* and *r.w.c.* is not one of the meagreness or fullness of the data to be evaluated, that is, is not a question of the number of pairs of stimuli used. But it is a question of the *degree of accuracy* aimed at. The *m.* of *r.w.c.* gives a relatively fine measure of precision, of which the mean error, probable error, and other quantities of the *m.* of *m.e.* are the bares and roughest indications. But so far as these last signify anything, they suggest *approximately* the steepness of that same curve (Fig. 3) of which the 'Präcisionsmass' of the *m.* of *r.w.c.* is a relatively *accurate* measure. The difference between the two mathematical methods is thus purely one of degree of accuracy, and it is a mere accident of technique that the *m.* of *m.e.* cannot be used with judgments on only one pair of stimuli, nor the *m.* of *r.w.c.* for the determination of not perceptible differences. In fact the latter is possible if an experimenter should choose to neglect a part of the wrong cases, taking only those which form the curve of 'equal' judgments (Fig. 1), and deriving the steepness of this curve by means of Fechner's table. For this would be a permissible variation of the *m.* of *r.w.c.*, comparable to the several variations in the *m.* of *m.e.* which have been used and recommended.

It may well be questioned whether the *m.* of *r.w.c.* is not a much more accurate procedure than the conditions of experimentation ever justify; or whether the application of least squares would not be a case of penny-wise after pound-foolish, in any sort of physiological work whatsoever. But we are not here concerned with the mathematical minutiae of method, — only with the general classification. The new tent poles are to carry all the old canvas. Indeed if mathematical details were here in question, it would be our first duty to examine and if possible to justify the fundamental assumption of both methods, *i. e.*, that the theory of errors may be applied to curves which never are and by all psycho-physic laws never can be, truly symmetrical.

To survey our results once more, the four psycho-physic 'methods' resolve themselves into the four problems, of finding the threshold of sensation, and the thresholds of not perceptible, just perceptible, and equal over-perceptible, difference; and

then the real two methods of mean error and right and wrong cases. The four original 'methods' are curiously illogical classes, which have come about in the course of the development of psycho-physics. They are historical relics. The first two 'methods' are only problems; the third is a problem and a method; while the fourth is a method but not a problem.

We have seen that in all psycho-physical experimentation there are two stages of the work — the getting of data, and the evaluating of them. We have already considered, so far as it lies in our purpose, the second, purely mathematical stage. It is instructive, and in part will account for the retention of the false methodology, to note how the experimenter has to approach the first stage of his work, the getting of data. Suppose that he wants to study by means of the ordinary olfactometer and one of the four 'methods' the discrimination for odors. He cannot use the 'method of mean gradations' because few if any subjects are able to identify and hold in mind an over-perceptible difference between odors. So the experimenter thinks that he must choose another 'method'; the fact is that he must choose another problem, that is, he must not hope to measure the olfactory discrimination for over-perceptible differences. Similarly he will believe himself deterred from the 'method of mean error,' because he has been taught that this 'method' involves the adjustment of the stimuli by the subject himself: and of course an odor-tube cannot be adjusted back and forth like a monochord. He may think of doing himself, as experimenter, the adjusting, continuing each time, although this is contrary to the school directions, until the subject declares a just not perceptible difference. This is possible to do, but hardly advisable; since it happens by an accident of physics that the olfactometer is more suitably and naturally adjusted from less olfactory stimulus to more, rather in the opposite direction. It is an accident, then, that the experimenter cannot well study just not perceptible differences, but must choose the remaining problem, the 'method' of just perceptible differences.

It may be said that this necessity of casting about for a problem (*n.p.d.*, *j.p.d.*, *e.o.p.d.*) suited to the accidental pecu-

liarities of the sense-organ to be studied and the apparatus at one's disposal, has made the spurious distinction between 'methods' seem real and practical to experimenters who seem not to have noticed that the distinction is in problem and not in method.

Thus the first stage of psycho-physical experimentation, the getting of data, is practically not determined by the experimenter, but by relatively accidental circumstances, — peculiarities of the sense-organ chosen and of the apparatus available. And in determining what data can be gotten, these accidents determine at the same time what problem can be studied, that is, whether *n.p.d.*, *j.p.d.*, or *e.o.p.d.* The word 'accident' may be objected to. Yet it is fair to call accidental the circumstance, for instance, that the *n.p.d.* cannot be studied in the active muscle-sense. Such seemingly chance and irrelevant factors are almost numberless. An important one of them, although it influences the general problem less directly, is the possibility of individual or massed stimulation of end-organs. By an anatomical accident the olfactory end-organs cannot be stimulated individually; so that what is an interesting problem of the dermal senses, the relative thresholds of different individual end-organs, cannot be studied in the sense of smell. It is apparently an accident, though an interesting one, that some sense-organs, as the olfactory, gustatory, or dermal when stimulated singly, do not afford us clear sensations of intervals, *i. e.*, of over-perceptible differences. Hence their power of discrimination must be studied in other respects; the problem of *e.o.p.d.* is debarred. Once again, if the experimenter happens to have two tuning-forks and nothing else, he will necessarily adapt his problem to the *m.* of *r.w.c.* with which he will have to evaluate his data. Whereas if he had a sonometer, he would more naturally let the subject adjust for himself and give judgments of *n.p.d.* The case in which the investigator has but two different stimuli to apply (and these must be nearly alike) is the one case in which truly the *method as well as the problem* is determined by irrelevant circumstances. Otherwise the *method* is chosen (*m.* of *m.e.* or *m.* of *r.w.c.*) according to the *degree of accuracy* which is desired.

Aside from the accidents which determine the problem, there is another kind which influences only the technique or at best bears but remotely on the problem. Such a factor, for instance, is the matter of simultaneity or succession of stimuli. Since the olfactory end-organs have to be stimulated all at once, any discrimination which is studied has to be a successive one (apart from the very doubtful case which some would claim, of simultaneous bilateral stimulation). This circumstance would still leave as possible any of the three problems; but it helps to restrict more precisely the technique to be adopted. There are countless other accidents of a similar sort.

Therefore in first approaching a psycho-physical problem, and in trying to get it realized in some arrangement of apparatus, the experimenter finds that very little is left to his free choice. Sometimes indeed he may choose no more than barely the sense which he studies. Two kinds of accidental circumstances, as we have seen, restrict his course, although in practice it is scarcely necessary to distinguish between them. In order to accept the inevitable, and set up his apparatus with the least waste of time and thought, the experimenter must run through the possible limitations and find out definitely what ones actually confront him. The possibilities are fairly well included under the following categories, although the list aims merely at being serviceable but not exhaustive.

1. Comparison — (*a*) simultaneous, (*b*) successive.
2. Comparison — (*a*) direct (immediate), (*b*) mediate.
3. Comparison between — (*a*) two stimuli, (*b*) more than two stimuli.
4. Variation — (*a*) random, (*b*) progressive.
5. Variation — (*a*) continuous, (*b*) discontinuous (step-wise).
6. Apparatus operated by — (*a*) the experimenter, (*b*) the observer.
7. Actual relation of the stimuli — (*a*) known to the observer, (*b*) not known to the observer.
8. (If the sense to be studied and the above enumerated conditions are such as still to leave the question open): Discrimination of — (*a*) threshold of stimulation, (*b*) not perceptible difference of stimulation, (*c*) just perceptible difference of stimulation, (*d*) equal over-perceptible differences of stimulation.

These headings are all familiar to the psychologist and need no elucidation. They are neither completely independent nor yet mutually exclusive. Class 1, for instance, is independent of 2; but 3, *a*, excludes 4 and 5. After learning what of these alternatives are open, the experimenter will see his way of proceeding rather precisely marked out. Herewith the first methodological stage is ended. Before coming to the second stage, of mathematical evaluation, he has only to get his data.

In regard to this second stage we have already seen that there are only two ways of evaluating data (although each method allows some minor variation), the methods of mean error and of right and wrong cases. It was not the purpose of this paper to discuss the methods in detail, but only to analyze the so-called four 'methods' and to classify the results. This we have now done. It need only be noted once more, that the choice between the two actual methods (except in the case of only two stimuli being used) depends on nothing but the degree of accuracy which is desired, that is, on the amount of labor which the experimenter thinks proper to devote to the inquiry. We may now pass to two of the other emendations of the traditional methodology, which have been offered.¹

¹The writer greatly regrets that before this article was actually set up, he had not seen the admirable work of G. E. Müller, in the *Ergebnisse der Physiologie*, 2ter Jahrgang, 1903, II Abth., SS. 266-516. Müller finds four cases (Fälle) in which psycho-physic methods may be used. These are our *t. of s.*, *j.p.d.*, *n.p.d.* and *e.o.p.d.* Of 'methods' he finds three; the first, in which the observer adjusts the variable stimulus in random sequence; the second, in which the experimenter does this, but in orderly increasing or decreasing progression; and the third, in which the stimuli are not adjusted (old method of right and wrong cases). There then remains the task (Aufgabe) of finding in general two values, a mean and its variability. This may be done immediately (our *m. of m.e.*), or by the mediation of formulæ (our *m. of r.w.c.*).

Of course this is in essentials far nearer to that which has been urged above than is any other classification hitherto offered. To the present writer it still seems, however, that the 'cases' are problems of which accidental circumstances largely determine the choice; that Müller's 'methods' are merely three among a large number of equally important such accidents; and lastly, that the actual methods are Müller's two Aufgaben, the treatment of the results either with or without formulæ. But only the second point is a difference in principle, while the first and last are merely nominal.

THE CLASSIFICATION OF KÜLPE.¹

Külpe's analysis of the methods is not fundamentally different from that of Wundt, but there are superficial differences which are worthy of a brief consideration. For Külpe there are two groups of methods, that of minimal changes and that of errors; these two correspond to Wundt's gradation and telling-off methods. The method of minimal changes has four 'applications'; while the error methods are two—the method of right and wrong cases and the method of mean error.

The method of minimal changes may be 'applied' to the determination of threshold (*Reizbestimmung*), to the comparison of stimuli (*Reizvergleichung*, the *n.p.d.* mentioned above), to the determination of difference (*Unterschiedsbestimmung* or *j.p.d.* above), and to the comparison of differences (*Unterschiedsvergleichung* or *e.o.p.d.* above). This virtually admits, although Külpe seems unconscious of the fact, that these four groups are not methods but problems, as has been argued in this paper. Furthermore his classification is symmetrical and consistent: *i. e.*—

1. Threshold—(*a*) of sensation, (*b*) of interval between sensations.

2. Equality—(*a*) of sensations, (*b*) of intervals between sensations.

Külpe well says that there is one method, which he calls the 'method of minimal changes, that applies to these four classes. This 'method' is essentially like the procedure described by Wundt under the same name, and is subject to the objection which was noted in the early part of this paper. This is, as will be remembered, that whether the compared stimulus varies continuously or step-wise, there is no point at which the change in judgment from being always 'greater' or always 'less' to being for the first time 'equal,' or vice versa, is truly significant. For let us suppose the compared stimulus to be decreasing toward the standard, it may be considerably greater than this and be judged 'equal' while when further decreased it will be again judged 'greater.' Or if it is increasing to the standard, it will often be judged 'equal' when considerably less than the

¹ Külpe, O., *Grundriss der Psychologie*, Leipzig, 1893, SS. 55-81.

standard, but again 'less' when it has been increased nearly to equality with the standard. To ignore these inconsistencies and to interrupt the comparisons with the first judgment of equality which is given (or inequality, as the case may be) is, if the compared stimulus varies step-wise, to take a measurement of discrimination which depends materially on the size of the steps which have been arbitrarily chosen. If the compared stimulus varies continuously, the case is a trifle better but not much, since then the measure of discrimination is considerably vitiated by fatigue and expectation (see above). Furthermore it is often not possible to make the compared stimulus vary continuously.

For these reasons Wundt virtually admits, as we have seen, that here is no method: and of his 'method of minimal changes' he leaves, although merely by implication, only the problem of the *j.p.d.* Külpe, however, accepts the informal procedure disparaged by Wundt, and insists that it is a method. It has been asserted elsewhere in this paper that every psycho-physical method must involve a procedure which yields at least a part of the data shown in Fig. 1, and must then mathematically evaluate from these some sort of a measure of discrimination. Now what part of Fig. 1 does this procedure yield which Külpe recommends? And how are these data evaluated? The 'method' is to present to the subject a pair of stimuli, a standard and a compared, for his judgment of 'less,' 'equal' or 'greater.' The compared stimulus varies in successive presentations not at random but so as to approach or depart from the size of the standard stimulus; and it may do both, either above or below this standard. Thus there are four modes in which the compared stimulus can vary. Whether in a given experiment some or all of these modes are used depends on the problem to which this 'method of minimal changes' is being applied.

Let us suppose, for example, that it is being applied to the problem of *j.p.d.* All four modes are used. The compared stimulus (*c.s.*) starts so much smaller than the standard stimulus (*s.s.*) that it is always judged smaller; and is gradually increased until the first judgment of equality is gotten. Here the progressive comparisons are interrupted, although if *c.s.* were

further brought up toward *s.s.* it would very likely be judged less once or twice more. Here is of course the weakness of this method. Now *c.s.* is taken equal to *s.s.*, and made gradually to decrease until the first judgment of less is given. So far the experimenter has a series of judgments 'less' 'less' et cet. — 'equal,' for *c.s.* increasing; and a series 'equal' 'equal' et cet. — 'less,' for *c.s.* decreasing. In both cases *c.s.* is smaller than *s.s.* But it can increase and decrease while larger than *s.s.* Therefore two more similar series are gotten, for values of *c.s.* above *s.s.* Curiously enough, Külpe writes as if it were enough to take the average of only these four values of *c.s.* at which the first change in the judgment occurred.¹ But he can

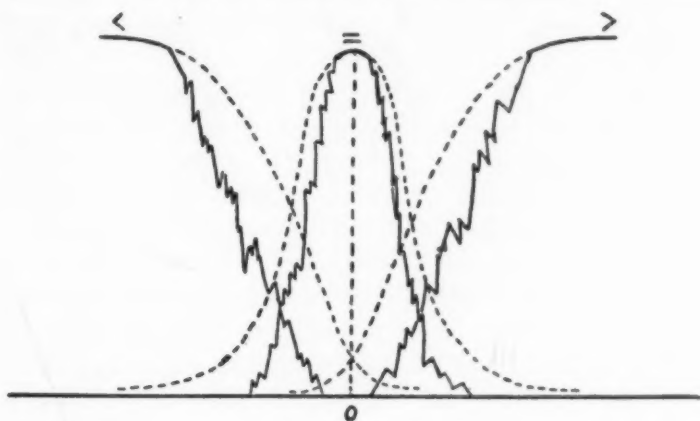


FIG. 4.

not mean so, for it appears later,² that a mean variation may be in question; so that of course, as one would expect, the four series are to be gotten many times, and an average of all taken for the actual measure of discrimination. The reader will see at once that if plotted these raw data would look like Fig. 4.

The relation to Fig. 1 (the dotted lines) is clear enough. Where the differences between *c.s.* and *s.s.* are so very small or so very large as always to be correctly told, the curves run smoothly. They become jagged where the chance errors interrupt the various series of 'less,' 'less,' et cet. —, 'equal,'

¹ Külpe, *op. cit.* SS. 60-62.

² *Ibid.*, SS. 66.

'equal' et cet.—, or 'greater,' 'greater' et cet.—, with a change in the judgment uttered. Now, as we have already seen, it is just because the first change in judgment depends more on the size of the step variations of *c.s.*, on fatigue or expectation, than on the fineness of discrimination in question that this method, as even Wundt grants, is not a proper method. It selects for further mathematical evaluation a certain portion of the data of Fig. 1; but the principle of selection is largely, though not utterly, independent of the power of discrimination which is being studied. And whereas in the *m.* of *r.w.c.* when only two stimuli are used, the selection although arbitrary yet leaves enough of the essential parts of the curves; the principle of selection here used effectually excludes just these indispensable parts, namely the parts which show the characteristic steepness of the curves.

Therefore in the earlier part of this paper the name of method was justly denied to this procedure, since it is absolutely inadmissible. And it was insisted that the only allowable and yet mathematically convenient method was to take data giving the complete curves of Fig. 1, and then to express approximately the steepness of those curves (transformed perhaps as in Fig. 3) by means of a mean error, probable error, or by some such readily obtainable quantity. Külpe's method of minimal changes is simply under no circumstances allowable. The merit of this part of his classification lies wholly in his four-fold 'application' of the 'method of minimal changes.'

The other part of his classification needs but brief consideration. As to his 'method of right and wrong cases,' he well says:¹ That it 'is capable of as manifold application as the method of minimal changes'; but then he goes on to describe the method so wholly in the traditional way that one has the impression that the use of only two stimuli is as essential to this method as is Gauss' equation itself.

Külpe's treatment of the 'method of mean error' adds nothing to that of Wundt, except the statement that while the method has been used only for the *n.p.d.*, it could be used for the *e.o.p.d.* as well. But he declares (S. 78) that it can be used

¹ *Ibid.*, S. 70.

only when experimenter and subject are one person, and when the stimulus can vary continuously. Both of these statements we have seen to be untrue.

THE CLASSIFICATION OF EBBINGHAUS.¹

The other classification which will concern us in this paper is that of Ebbinghaus, given after his short and admirable account of the traditional four methods. Ebbinghaus says (S. 75) that, 'The goal of psycho-physic method is the determination of those stimulation values which are the outward causes of equal-seeming psychic values (that is, of equal intervals between sensations),' of course, then, the threshold of stimulation is not a problem of psycho-physics; and in fact the author makes no mention of it.

Now as to the size of the intervals between sensations, it can be either just perceptible or over-perceptible, and if the latter it can be as large as one chooses. Herewith our problems of *j.p.d.* and *e.o.p.d.* are recognized, although still called methods; while the problem of *n.p.d.* is ignored. Secondly, as to the way of judging, this may be so chosen that the subject (S. 75) 'has in mind the idea of an equal interval and varies the outward stimulus until the sensation which it gives corresponds to this idea: or, one can present repeatedly for judgment a given pair of stimuli and let the subject give his judgment in terms of certain replies previously agreed on,' such as 'less,' 'equal,' and 'greater.' The first procedure is called the 'finding a stimulus' to correspond to a given judgment; the second, the 'finding of judgment' to correspond to a given pair of stimuli. Lastly, in every case the final result must be the average of many observations; and since each observation will vary from this average, the mean of the variations must always be given (mean deviation), in order to show the reliability of the total average.

This scheme is very original and suggestive; but inadequate. It recognizes but one true method, the one which we have called above the *m.* of *m.e.*; and but two problems (which Ebbinghaus still calls methods) to which this one method can

¹ Ebbinghaus, H., *Grundzüge der Psychologie*, Leipzig, 1902, SS. 74-80.

be applied; those are our problems of *j.p.d.* and *e.o.p.d.* The problem of threshold of stimulation is dismissed as not lying within the field of psycho-physics. The expediency of so narrow a definition of psycho-physics is doubtful; for it should seem better to account the problem of psycho-physics the measured correlation in general of stimulus and sensation; and thus to include the problem of threshold, which after all must be studied if at all by the *m.* of *m.e.* or that of *r.w.c.* Also the problem of *n.p.d.* finds no recognition because, as the author says (S. 78): 'The third method, finally, that of mean error [that is the third traditional method, by which he refers to the problem of *n.p.d.*], affords nothing directly which can be utilized as a measure of sensation, since it operates not with a sensation interval, but with the disappearance of an interval'; *i. e.*, it operates with a not perceptible interval, which is seemingly not to the point. And again he says (S. 69): 'The determination of this mean error [made in judgments of *n.p.d.*] through the range of a given sense has of course its use, but clearly these values are something quite different from equal sensation intervals in the above mentioned sense [that is, of just perceptible, and equal over-perceptible, intervals], and the process of getting them is no true measuring of sensation but a process having some relation to such a measuring.' Ebbinghaus gives elsewhere his grounds for this view, in that the *n.p.d.* never had any value except on the assumption that it bears a definite and fixed relation to the *j.p.d.* from the same standard stimulus. This assumption has not been shown to be valid, and Ebbinghaus seems to account it very speculative if indeed not certainly false. He may be quite right. The point is one more of mathematical technique than of classification. If he once granted the validity of the *n.p.d.*, Ebbinghaus would doubtless class it with the *j.p.d.* and *e.o.p.d.*

The analysis given in this paper found the third so-called 'method' of tradition to consist in a problem and a method. Ebbinghaus discards the problem but retains the method, which is our *m.* of mean error. This is in fact his one method, and it must be used in every one of the four procedures into which he resolves the traditional 'methods.' These are, once more:

1. Just perceptible interval with finding of stimulus.
2. Just perceptible interval with finding of judgment.
3. Over-perceptible interval with finding of stimulus.
4. Over-perceptible interval with finding of judgment.

Thus in every psycho-physical procedure an average of the observations must be taken, with their mean variation or error. These two quantities are the measure of discrimination which is desired.

But what of the 'Präcisionsmass' as yielded by Gauss's equation and Fechner's table of integrals? Ebbinghaus minimizes in his system this method of evaluation because, it should seem, he accounts it a very laborious process to be used only when all the observations are based on but two stimuli: the more cumbersome mathematical evaluation being used solely in order to make up for the poverty of the material of observation. The author does not admit, apparently, that here is an evaluation method of relatively great accuracy, which can be applied as well to observations on many pairs of stimuli as to those on only one pair.

We have seen that from the first and second traditional methods Ebbinghaus analyzes out his two categories of just perceptible and over-perceptible, difference. Both of these 'methods' proceed by '*finding the stimulus*,' whereas the fourth 'method' (of right and wrong cases) proceeds by '*finding the judgment*'; hence these two new categories. Every psycho-physical procedure uses one of the last two, together with one of the first two categories. Thus arise the four methods of Ebbinghaus, given on the preceding page. But now all four of these methods have to use the little germ of truth which lay in the traditional third 'method' (of mean error): this was the use of the average and its mean error. Wherefore the final form of Ebbinghaus's four methods is as follows:

1. Just perceptible interval with finding of stimulus; average of all observations with *m.e.*
2. Just perceptible interval with finding of judgment; average of all observations with *m.e.*
3. Over-perceptible interval with finding of stimulus; average of all observations with *m.e.*

4. Over-perceptible interval with finding of judgment ; average of all observations with *m.e.*

Thus this system, which is somewhat simpler than the traditional four, is gotten by the omission of two problems, those of the threshold of stimulation and of the *n.p.d.*; and by the omission of one method, that of right and wrong cases. It is curious that aside from the relatively special and insignificant expedient for evaluating judgments on only two stimuli, Ebbinghaus finds nothing in the traditional method of right and wrong cases save the suggestion to fit the judgment to the stimulus instead of the stimulus to the judgment, as in minimal changes.

Although this classification leaves out so much that it becomes inadequate to the subject, it probably has the distinguished merit of being the first radical and strictly logical recasting of the methodology. It is well worthy of study, and will be found to be possibly, the clearest and best form for teaching the methods of psycho-physics to elementary students.

SUMMARY.

The four traditional methods of psycho-physics are found to be an illogical scheme of a subject which if analyzed resolves itself into the following system.

I. Four problems as to the relation between stimulus and sensation, which admit of quantitative treatment.

- (a) The threshold of stimulation (*t. of s.*).
- (b) The not perceptible difference of stimulation (*n.p.d.*).
- (c) The just perceptible difference of stimulation (*j.p.d.*).
- (d) The equal over-perceptible difference of stimulation (*e.o.p.d.*).

II. A great diversity of procedure by which data on these problems are gotten (see above), and of which the one thing essential is that judgments shall be obtained which group themselves like a part or like the whole of the judgments in Fig. 1, and so that the steepness of at least one of the curves of Fig. 1 is implicitly contained in the judgments. This variety of procedure is not a variety of choice open to the experimenter. The procedure used in any case depends on relatively acci-

dental characteristics of the sense-organ chosen for study; and for this reason they are not susceptible of rigorously logical classification. Furthermore it generally happens that these different accidents, so far from being a source of freedom, actually restrict the experimenter in his choice of problem, so that he is obliged to study a particular one of the four problems.

III. Two real methods by which the judgments can be evaluated into a measure of the discrimination of the sense studied: that is, by which the steepness of one or all of the curves in Fig. 1 can be approximately or accurately expressed. These may be called, out of respect to tradition.

(a) The method of mean error (*m. of m.e.*).

(b) The method of right and wrong cases (*m. of r.w.c.*).

The difference between these is one of accuracy. The former method measures the precision roughly by taking an average of the observed values and their mean error, or probable error, et cet. The latter method is more refined and uses the equation of Gauss and the integral tables of Fechner to obtain a 'Präcisionsmass.' The significance of either of these measures lies in its being an index of the steepness of one or all of the curves of Fig. 1.

Either of the two methods may be used with any of the four problems. There are thus eight alternatives. Having chosen the sense-organ which he will study, the experimenter finds his mode of procedure limited in many respects by accidental peculiarities of this sense and of the apparatus at his disposal. He may then choose what of the four problems he will study, or he may find even this determined by the accidental peculiarities. He is, however, free to choose his method of evaluation: and the basis of his choice is nothing but the degree of accuracy which he desires in his results or, what generally comes out to the same thing, the amount of labor which he is willing to spend on them. The one exception is the case in which some circumstance limits him to the use of only two stimuli, for then it limits him also to the *m. of r. w. c.*¹

¹ The MSS. of this article was received April 17, 1904.—ED.

STUDIES IN THE INFLUENCE OF ABNORMAL POSITION UPON THE MOTOR IMPULSE.

BY DR. CHARLES THEODORE BURNETT.

I. THE JAPANESE ILLUSION AND THE MIRROR ILLUSIONS.

The experiments that form the basis of the following report are the first of a series designed to open a new approach to the psycho-physics of the motor impulse, by way of the modifications that occur in the control of a limb when placed in unusual positions. The particular investigations of this paper are concerned with the direction of the impulse as shown in the control of the fingers when the hands are placed in unusual positions; concerned, in other words, with the ability to move a given finger at command.

We shall consider first the Japanese illusion. It occurs when, with arms crossed, the hands are clasped thumbs down, and are turned thumbward till they point up. If an onlooker, pointing to one of the fingers, asks the man thus situated to move it, the latter is frequently unable to do so, moving, if anything, some other finger. Of the experimental conditions, it need be said only that the hands were unclasped after every movement in many series; and that either the wrists and neighboring parts of the arms were concealed by a cloth wrapped about them, or the observer was covered with a sort of apron fastened about the neck and having an opening with edges drawn together by an elastic cord. Through this opening the clasped fingers could be thrust while most of the remainder of the hand was concealed. The purpose of these precautions was to preserve as long as necessary an illusion that yields pretty readily to experience of the situation. In the first form of this experiment the finger to be moved was indicated visually to the observer by pointing, whereupon the latter was to make the movement as quickly as possible. No attempt was made to eliminate the possible influence of the crossing of hands, whether right over left or left over right. But here and through-

out, except in a single instance noted in its place, each finger (including thumbs) was as often required to move as is any other. The order of choice was irregular.

TABLE I.
JAPANESE ILLUSION.

	Date.	No. Exper.	Total Errors.	Symmet. Opposite.	Pairs.	Next Finger Opposite.	Next Finger Same.	Miscell. Opposite.	Miscell. Same.	Right. ¹	Left. ²
Baldwin.	9 Oct. ¹	40	22	15	5	1	1				
	16 "	60	14	5	5		4			7	7
	23 "	20	1				1				1
Emerson.	28 "	60	29	16	2	4	5		2	14	15
	4 Nov.	80	35	19	1	8	7			8	27
Kleinknecht.	3 Dec.	40	19	15		4				8	11
Miller.	28 Oct.	60	23	14	1	6	2			3	20
	4 Nov.	60	15	12		1	2			2	13
Rouse.	16 Oct.	40	20	13		4	1	2		3	17
	23 "	40	19	16		3					19
	30 "	60	25	21		1	3			3	22
Rowland.	28 "	40	14	7		2	4		1	5	9
	4 Nov.	60	26	16		4	6			4	22
Totals.		620	240	154	9	37	35	2	3	57	183

RESULTS.

1. The disturbance in the direction of the motor impulse is rather large, as shown by the proportion of errors to the number of experiments.

2. In some cases adjustment to the abnormal position is not long in occurring. Miller and especially Baldwin show this. The latter, in a short test made after those recorded, showed entire readjustment.

3. The erroneous movements far more frequently occur in the finger symmetrically opposite than in any other. Following at a long distance are erroneous movements in the next fingers opposite and the next fingers on the same side. Errors of other types are few and scattering.

4. The errors occur far more frequently when the movement is to be made with the left hand than when it is to be made with the right. No observer shows a contrary tendency, though some exhibit none.

¹Omitted from totals because fingers were not equally employed.

²In these columns throughout the tables are recorded the number of *failures* for the hand in question.

5. With hands in normal position, palms up, there are practically no errors.

6. When a finger is touched as well as pointed out, there is almost never an error.

7. When the eyes of the observers were closed and the finger to be moved was indicated by naming it, the following results were obtained:

	No. Exp.	Errors Symmet. Opp.	Errors Miscell.
Baldwin.	40	0	5
Emerson.	30	0	0
Kleinknecht.	30	0	4
Miller.	60	9	4
Rouse.	40	8	5
Rowland.	40	7	0

Here is a great reduction in the illusion. That this is not due in all cases merely to a growing familiarity with the situation is shown by the results of Emerson. Work in connection with this illusion previous to the present test had not occurred for four weeks. The day after this test the old conditions were restored and the illusion was back as strong as ever. The results of Miller show about as much illusion as in one of the sets of experiments recorded against him in Table I. Hence it seems possible that his confusion lay in his kinæsthetic knowledge of where his fingers were located. This confusion nearly disappeared for him when the hands were laid palm downward on the table pointing away from the body, while the other conditions of auditory stimulus and closed eyes were maintained. The presence of a weakened illusion with Rouse is perhaps connected with the fact that he visualized his hands. This Miller did not do.

CONCLUSIONS.

1. It appears from (6) and (7) above that the illusion lies in the visual, not in the kinæsthetic, experience of abnormal position, though one observer presents a possible exception.

2. The large excess of erroneous movements made with the finger symmetrically opposite shows how large a factor in the direction of the motor impulse is the visual peculiarity of a given finger. The motor current appropriate to that peculiarity is

started; but the element contributed by visual position diverts it to the wrong hand.

3. Why, it may be asked, does the visual experience of abnormal position divert the current far more frequently to a finger on the opposite hand than to another on the same hand? A glance at the hands in the position appropriate to the illusion will show that the roots of the fingers lie on the side opposite to the arm to which they belong; that the right-hand fingers point *from* the left *to* the right, and the left-hand fingers *from* the right *to* the left. This is just the reverse of what is true when the hands are clasped in the usual way. Going upon the basis of procedure in the normal situation, the observer in the unusual position moves the finger that *really* lies on the side on which the given finger *appears* visually to lie. This process of reasoning is, of course, wholly in the mind of the experimenter. For the observers the process is so mechanical that they are obliged to consider seriously when asked how they obey a given command. The usual reply is that they simply *see* what is wanted and then do it. The movement appears to follow directly upon the visual cue. It is not to be denied that the observers feel in some measure confused in this unusual position and occasionally feel almost unable to move any finger. The attitude of hasty attention that favors so many geometrical-optical illusions seems to be the best one in the present instance. The confusion soon yields far enough to permit a movement that is not merely spasmodic.

4. Why any correct movements at all? They become possible by a new adjustment to the new position—a recognition that the right-hand fingers point *from* the left and the left-hand fingers *from* the right. Some effort may be required to substitute the new visual cue for the old, and, when effort fails, habit steps into control. The new adjustment may be but partly successful and a wrong finger moved on the correct side. The mistakes of this sort give the second maximum of errors.

5. Is there any psychological account to be given of the second focus of errors in Table I., viz., in movements of the fingers *next* to the correct one whether on the same or the opposite hand? This is possible if in some way it could be shown that

the two fingers resembled each other. The middle and ring fingers resemble each other more than do any other two adjacent members, and the thumb and forefinger least of all. Here is the way in which the errors were distributed among adjacent pairs:

Thumb and fore-finger = 19
Fore- and middle-finger = 19

Middle and ring = 25
Ring and little = 10

There is no ground here for basing the error wholly on mutual resemblance, though to this it may at times be due. We seem driven to a purely physiological account.

6. That the second greatest tendency to error should involve moving a finger *next* to the correct one, while yet this tendency cannot be due in general to resemblances, suggests that what would be the habitual course of the motor impulse is preventing somehow a wider divergence in its actual course. It does not appear otherwise why the errors should not be more widely distributed.

7. The source of superiority in control of the right over that of the left hand does not at once appear. For movements so simple in the normal position such a difference does not exist.

TABLE II.

JAPANESE ILLUSION.

Left hand crossed over right.

	No. Exper.	Errors.	Symmet. Opposite.	Next Finger Opposite.	Next Finger Same.	Right. ¹	Left. ¹
Emerson.	40	23	15	8		14	9
Kleinknecht.	40	14	14			6	8
Rouse.	40	5	3	2			5
Rowland.	40	16	11	3	2	2	14
Totals.	160	58	43	13	2		

Right hand over left.

	No. Exper.	Errors.	Symmet. Opposite.	Next Finger Opposite.	Next Finger Same.	Right. ¹	Left. ¹
Emerson.	40	19	14	5		6	13
Kleinknecht.	40	17	16	1		3	14
Rouse.	40	12	10	1	1	5	7
Rowland.	40	18	14	3	1	5	13
Totals.	160	66	54	10	2		

¹ Totals henceforth are not recorded in these columns because of the divergence among observers.

One objective factor not thus far controlled might be involved in this result. The abnormal position studied here can be obtained by crossing right hand over left or left over right. There is frequently a difference in strain in the two wrists; and the hand and wrist of more intense sensation might possibly be under better control. So much is at least suggested by the lessening of error when the control was of the auditory-kinæsthetic type. Or we might indeed find the reverse to be true. Table II. gives us the results of experiments similar to the preceding, but designed to show the effects, if any, of the method of crossing. Baldwin is omitted in this test because the illusion had nearly disappeared for him.

RESULTS.

1. There are still many more failures in case of a commanded movement with the left hand than with the right. There is but one observer whose results suggest any influence of the method of crossing. No simple relation is apparent between the presence or absence of a feeling of strain, as reported by the observers, and this particular tendency to error. So the cause must still be sought.

2. The distribution of errors is like that in Table I, except that all scattering errors have disappeared and very few are found in the next finger on the same side.

MIRROR EXPERIMENTS.

In the following sets of experiments the abnormal position was attained by the use of a mirror, occasionally of two. The mirror space inverts the spaces of the real world in a direction perpendicular to the plane of the mirror; so that fingers in front appear to be in the rear, and those to the right lie apparently on the left; and *vice versa*. A direct view of the fingers was prevented by a broad collar of cardboard. After a few of these experiments had been made it was thought best, to the end of preserving the illusions in force, that the observers either close their eyes or look away after noting the finger to be moved, and then complete the movement. They were forbidden, however, to develop any new sources of information after closing the

eyes. The general conduct of the experiments was as before except that the thumbs were not used, since in some positions they could not be conveniently interlocked with neighboring members.

The results are so arranged in the tables as to show the extent to which the errors follow the mirror reversal. To illustrate—when the hands are clasped palms up and the line of the interlocked fingers is perpendicular to the plane of the mirror, the forefingers which are really farthest from the body will in the mirror space be nearer the real body, while the little fingers, which are really nearer, will in the mirror space be farther away. If now the observer be directed by pointing to move a forefinger and he thereupon move the little finger or ring finger, that error would show that the movement followed upon the visual cue, the mirror space being regarded not otherwise than as real space. An erroneous movement of fore- or middle-finger for either ring- or little-finger will be classified thus; not so fore- for middle-finger or *vice versa*, nor ring- for little-finger. In other words, the eight fingers, being interlocked, are divided by a median line into two sets. The finger wrongly moved must not lie in the same half with the finger pointed out, if the error is to be classed as following the mirror reversal. If it does lie in the same half, one cannot say that the error is *not* due to the same cause. But by arbitrarily limiting the evidence to the more striking cases, a preponderance of these will make our conclusions much stronger.

By way of introduction we may notice here the character of the errors occurring in the attempt to trace with a pencil the outlines of figures that cannot be seen directly but only as reflected in a mirror. Henri¹ reports such experiments. The present results confirm his in all essential respects. (1) When asked to trace the outlines of a rectangle whose side was parallel to the plane of the mirror, all seven observers succeeded easily, though in four a false start in the opposite direction was noted when they began to trace the lines perpendicular to the mirror. This is the space relation that the mirror reverses, and

¹ 'Revue generale sur le sens musculaire.' V. Henri. *Année Psych.*, V., pp. 504-8.

the wrong movement thus conformed to the visual cue. (2) The tracing of the diagonals in this position was almost, if not quite, an impossibility for four observers, movements being made *at right angles* to the one desired, *i. e.*, *in a direction conforming to that of the reflected line*. For the other three observers the movement was easy enough except at high speeds, where an occasional error similar to the foregoing betrayed the tendency usually held in check by the successful adjustment to the new conditions. The reaction seems to involve the association of a new kinæsthetic complex with a given visual impression as soon as the reflected image shows that the movement is being made in the right direction. These two types of reaction suggest two types of brain function—the one where the organic paths already formed chiefly determine the direction of the motor impulse, and the presence of an element common to the new and the old is sufficient to draft the entire current into the old channels; while in the other type *all* the new elements contribute in determining the direction of the motor impulse. (3) When the corner of the rectangle was toward the mirror, the difficulty in drawing sides and diagonals respectively was reversed; but in kind was like the earlier error. (4) If a more complicated figure, such as a six-pointed star, be set for outlining, the difficulty increases, though in the case of one or two observers all

TABLE III.

MIRROR FRONT. SUPINATION. FINGERS CROSSED IN PALMS.

	No. Exper.	Errors.	Following Mirror.		Next Finger Same.	Miscell. Same.	Symmet. Opposite.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.							
Baldwin.	48	25	22	1			2			11	14
Emerson.	72 ¹	33	19		10		4			17	16
Kleinknecht.	24	2			2					1	1
Miller.	72 ¹	45	30	4	8		2	1		22	23
Rouse.	40	34	21	5	7		1			15	19
Rowland.	88 ¹	26	7	1	4		10	4		16	10
Totals.	344	165	99	11	31		19	5			

¹ Results of several days combined. Tendency in the separate series the same as that in total except in Rowland's failures to right and left. The excess of right-hand failures is due to the results obtained at a single sitting.

the new adjustments desired throughout this experiment were made with ease.

TABLE IV

MIRROR FRONT. PRONATION. FINGERS CROSSED OVER BACKS OF HANDS.

	No. Exper.	Errors.	Following Mirror.		Next Finger Same.	Symmet Opposite.	Next Finger Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.					
Baldwin.	40	9	6		2	1		4	5
Emerson.	40	2			1	1			2
Kleinknecht.	40	3	2		1			1	2
Rouse.	40	8	1		5	2		4	4
Rowland.	40	11	1	2	1		7	1	10
Totals.	200	33	10	2	10	4	7		

Of the special conditions governing the experiments of Tables III. and IV. it need only be said that in both cases the line of the fingers was kept perpendicular to the plane of the mirror, so far as comfort would allow. The fingers were so clasped in the work of Table IV. that the left forefinger always came next to the body; while for the experiments of Table III. the left little finger occupied that place, except in a part of the tests with Emerson and Rowland.

RESULTS.

1. The disturbance in the direction of the motor impulse is very markedly shown in Table III., though one observer is almost unaffected.

2. The influence of the visual factor appears in the fact that more than two thirds of the errors follow the mirror reversal.

3. There is no prominent tendency toward an excess of failures in one hand over the other. For most of the observers it is quite absent.

4. Nearly one fifth of the errors consists in a movement of a finger of the opposite hand. This is not due to any inversion effected by the mirror, so far as one can see.

5. The bulk of all the errors not directly accounted for by the mirror reversal consists in the wrong movement of the symmetrically opposite finger and of the next finger on the same side.

6. Under the conditions of Table IV. the illusion has greatly decreased. It is to be especially noted that the causes operative

in the former case to produce errors that the mirror reversal could not *directly* account for are now much more effective. The mirror errors are about one third the total, while in Table III. they are more than two thirds.

7. The errors, barring those of a single observer, show no tendency to concentration in either hand.

TABLE V.

TWO MIRRORS, IN FRONT AND BELOW. SUPINATION. FINGERS CLASPED OVER BACKS OF HANDS.

	No. Exper.	Errors.	Following Mirror.		Next Finger Same.	Next Finger Opposite.	Symmet. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.					
Baldwin.	40	1	6		1	1		3	5
Emerson.	40	16	15		1			10	6
Kleinknecht.	40	16	11		5			8	8
Rouse.	40	21	10	2	1	1	7	5	16
Rowland.	40	12	4	1	6		1	2	10
Totals.	200	73	46	3	14	2	8		

TABLE VI.

TWO MIRRORS, IN FRONT AND BELOW. PRONATION. FINGERS CLASPED IN PALMS.

	No. Exper.	Errors.	Following Mirror.		Next Finger Same.	Next Finger Opposite.	Symmet. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.					
Baldwin.	40	16	10	1	1	1	3	6	10
Emerson.	40	11	10				1	5	6
Kleinknecht.	40	8	5		1		2	6	2
Rouse.	40	26	6	7	3		10	7	19
Rowland.	40	9	4				5	4	5
Totals.	200	70	35	8	5	1	21		

8. What is the cause for the great difference in the amount of illusion between Tables III. and IV.? A suggestion readily occurring would attribute it to the greater ease of recognizing the fingers as individuals when they are clasped over the backs of the hands. In support of this view may be cited the results of some experiments performed on Rouse. The conditions differed from those of Table IV. in this, that the fingers were covered with paper rolls that largely concealed their individual

characteristics. In the same number of experiments his errors were three times as many; and more than two thirds of these followed the mirror reversal. And these results were obtained a week after the former, so that the former results do not appear to have been due to practice.

But we shall find in Tables V. and VI. evidence to show us that the positions of pronation or supination can importantly modify the illusion; and so to these factors in the present case we shall have to allow some influence.

For the experiments of Table V. two mirrors were used, at right angles to each other; one flat, the other perpendicular to the median plane of the observer. The fingers were clasped in the manner indicated by the tables and directed downward, so that the observer looking into the upright mirror could see a reflection of the image of the flat mirror. A cloth over the top of the upright mirror prevented a direct reflection of the hands in it. The image as seen by the observer reversed the real position of pronation or supination and also, as in the preceding experiments, the halves of each hand. In all essentials the conditions of Table V. resemble those of Table IV., the conditions of Table VI. those of Table III., except in pronation and supination.

RESULTS.

1. The total amount of errors is greater in the position of supination than of pronation. In other words, the pronated hand appears to be under better control. The results of Miller have to be excluded from Table III. in order to make a justifiable comparison. Specifically stated, the errors for Table V., (supination) are more than double those in Table IV. (pronation). A comparison of Tables III. and VI. yields similar results.

2. Again the errors find a second center in Table V., in the fingers next to the one indicated and on the same side; while in Table VI., this second center is rather in the finger symmetrically opposite.

3. The tendency of the movement to follow the visual cue is still evident.

TABLE VII.

A. MIRROR FRONT. LEFT PALM UP. RIGHT DOWN.

	No. Exper.	Errors.	Following Mirror.			Next Finger Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.	Symmet. Opposite.					
Baldwin.	48	28	16	1	5	5		1	13	15
Boswell.	72 ¹	31	20		5	3	2	1	5	26
Emerson.	40	17	11		2	2			6	11
Holt.	24	13	12			1			2	11
Kleinknecht.	24	4	3			1			1	3
Rouse.	48	35	22		1	10	2		17	18
Rowland.	48	18	9			3	6		3	15
Miller.	48	26	16		4	4	2		9	17
Totals.	352	172	109	1	17	29	12	2		

B. AS ABOVE, EXCEPT RIGHT PALM UP, LEFT DOWN.

Baldwin.	48	15	8	1	2	2	2		11	4
Boswell.	72 ¹	37	20	1	5	11			11	7
Emerson.	40	13	11			2			9	4
Holt.	24	15	13		1	1			7	8
Kleinknecht.	24	5	1		2	2			4	1
Rouse.	48	31	21		4	4	2		19	12
Rowland.	48	29	11	1	9	3	3	2	14	15
Miller.	48	21	9		6	4	2		13	8
Totals.	352	166	94	3	29	29	9	2		

TABLE VIII.

A. MIRROR FRONT. DIRECTION OF FINGERS OPPOSITE. LEFT PALM UP, RIGHT DOWN.

	No. Exper.	Errors.	Following Mirror.			Next Finger Same.	Miscell. Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.	Symmet. Opposite.						
Baldwin.	40	25	23	1	1					8	17
Emerson.	40	18	17					1		8	10
Kleinknecht.	40	7	4					3		7	
Rouse.	40	22	13	1	3	4		1		8	14
Rowland.	40	18	12	1	1	2		2		3	15
Totals.	200	90	69	3	5	6		7			

B. AS ABOVE, EXCEPT RIGHT PALM UP, LEFT DOWN.

Baldwin.	40	16	13		1			2		9	7
Emerson.	40	19	16		2			1		8	11
Kleinknecht.	40	16	14			2				2	14
Rouse.	40	26	11		12	3				11	15
Rowland.	40	21	15	2	3			1		11	10
Totals.	200	98	69	2	18	5		4			

¹ These results are combined from the work of two days, agreeing in tendency.

TABLE IX.

A. MIRROR FRONT. CAPS ON PALM-DOWN FINGERS. LEFT PALM UP, RIGHT DOWN.

	No Exper.	Errors.	Following Mirror.			Next Finger Same.	Miscell. Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.	Symmet. Opposite.						
Baldwin.	40	22	15		2	3		2		8	14
Emerson.	40	8	8							1	7
Kleinknecht.	40	16	12			4				16	
Rouse.	40	24	19		3	2				13	11
Rowland.	40	9	8			1				5	4
Totals.	200	79	62		5	10		2			

B. AS ABOVE, EXCEPT RIGHT PALM UP, LEFT DOWN.

Baldwin.	40 ¹	22	18			3			1	12	10
Emerson.	40 ¹	13	11			2				9	4
Kleinknecht.	40	13	7		3	3				4	9
Rouse.	40	24	18		1	4		1		8	16
Rowland.	40	23	20			1		2		9	14
Totals.	200	95	74		4	13		3	1		

TABLE X.

A. MIRROR FRONT. CAPS ON ALL FINGERS. LEFT PALM UP, RIGHT DOWN.

	No. Exper.	Errors.	Following Mirror.			Next Finger Same.	Miscell. Same.	Next Finger Opposite.	Miscell. Opposite.	Right.	Left.
			Same Hand.	Opposite Hand.	Symmet. Opposite.						
Baldwin.	40	18	16		1	1				7	11
Emerson.	40	18	17			1				8	10
Kleinknecht.	40	13	13							11	2
Rouse.	40	18	15		2	1				3	15
Rowland.	40	14	9			2		3		1	13
Totals.	200	81	70		3	5		3			

B. AS ABOVE, EXCEPT RIGHT PALM UP, LEFT DOWN.

Baldwin.	40	20	17		2	1				9	11
Emerson.	40	19	19							15	4
Kleinknecht.	40	28	20			8				10	18
Rouse.	40	27	24		1	2				12	15
Rowland.	40	18	14	1	3					12	6
Totals.	200	112	94	1	6	11					

¹ Results obtained at two sittings but accordant.

A further test of the influence of pronation and supination, as well as of the visual position of the members, was devised in these new experiments.

The conditions belonging to them are the following: The line of the crossed fingers is again perpendicular to the mirror plane; but the clasped hands are one in the position of pronation, the other in that of supination. The differences among these four sets are the result of an attempt to eliminate the factors that might be responsible for the tendency to mass failures in a given hand. So in Table VIII. care was taken that the fingers of the supinated hand should not be allowed to curl up, as they are inclined to do; but should maintain their direction as steadily as do the fingers of the other hand. In Table IX. the attempt was made to check the one-sidedness that might well grow out of the greater ease in recognizing fingers whose backs are in view, by covering those fingers with caps made in the form of paper tubes. And these coverings were extended to the fingers of both hands in Table X., as equalizing most fairly the conditions for both. Here also the effort was made to maintain the opposition in direction of the fingers. Finally, the same number of experiments was performed with each hand in a given position.

RESULTS.

1. There appears at first sight to be no simple relation between the conditions studied and the tendency to mass failures in one hand. Looking further, however, we find that *while frequently there is no such tendency*, yet when it does occur, *the drift is to the supinated hand*. Cf. Table XI. One observer is a definite and consist exception.

2. This must be at least relatively independent of ease in recognizing the fingers, since it occurs even when the caps are on both hands.

3. Tables VII. and VIII. show a massing of erroneous movements on the symmetrically opposite finger, as well as on the next fingers of both the same and the opposite sides. This tendency to a confusion of hands cannot be accounted for as a case of mirror reversal. In Tables IX. and X., there is no such drift upon the symmetrically opposite finger, but the next fingers on the same side are chiefly favored.

4. In general, the distribution and the significance of the errors here agree with Tables III. and IV. A test of the fingers in this position under the condition of direct vision showed practically complete control.

TABLE XI.

SUMMARY OF TABLES VII.-X.

	Table VII.			Table VIII.			Table IX.			Table X.		
	Right.	Left.	Comment.	Right.	Left.	Comment.	Right.	Left.	Comment.	Right.	Left.	Comment.
Baldwin.	13	15		8	17	l. up	8	14	l. up	7	11	
	11	4	r. up	9	7		12	10		9	11	
Emerson.	6	11	l. "	8	10		1	7	l. "	8	10	
	9	4	r. "	8	11		9	4	r. "	15	4	r. up
Kleinknecht.	1	3		7		r. down	16		r. down	11	2	r. down
	4	1		2	14	l. "	4	9	l. "	10	18	l. "
Rouse.	17	18		8	14	l. up	13	11		3	15	l. up
	19	12	r. "	11	15		8	16	l. down	12	15	
Rowland.	3	15	l. "	3	15	l. "	5	4		1	13	l. "
	14	15		11	10		9	14	l. "	12	6	r. "
Boswell.	5	26	l. "									
	21	16	r. "									
Holt.	2	11	l. "									
	7	8										
Miller.	9	17	l. "									
	13	8	r. "									

Against the significant differences in Table XI. is indicated the hand that was supinated. All the observers, except Kleinknecht agree in concentrating failures, if anywhere, in the hand whose palm is up. There are but two exceptions in the twenty-two cases. Kleinknecht is just as constant in the opposite direction and furnishes a larger number of significant cases than does any other observer. Why the cause operating in the other observers should produce intermittent effects does not so far appear.

The conditions prevailing in these new experiments were calculated to increase yet more the influence of the abnormal visual position of the fingers. Two mirrors were set together at an angle of about 90° . The observer sat over against the apex of the angle thus formed, and his clasped hands lay in the region embraced by the angle of the mirrors. The manner of clasping the hands is shown in the table. The fingers were

TABLE XII.

A. TWO MIRRORS, RIGHT-LEFT, CAPS ON ALL FINGERS, LEFT PALM UP, RIGHT DOWN.

	No. Exper.	Errors.	Following Mirror.					Next Finger Same.	Right.	Left.
			Front-back and Right-left.		Front-back Only	Right-left Only.				
			Symmet. Opposite.	Miscell.		Next Finger Opposite.	Miscell. Opposite.			
Baldwin.	40	32	11	1	3	16		1	13	19
Emerson.	40	38	20		1	15		2	18	20
Kleinkuecht.	40	40	9	4		20	7		20	20
Rouse.	40	36	16	5		14	1		16	20
Rowland.	40	32	10		3	15	1	3	12	20
Totals.	200	178	66	10	7	80	9	6		

B. AS ABOVE, EXCEPT RIGHT PALM UP, LEFT DOWN.

Baldwin.	40	29	9	1	11	7		1	17	12
Emerson.	40	40	20	3	2	15			20	20
Kleinknecht.	40	39	18	3	1	16		1	20	19
Rouse.	40	39	22	3	3	9	2		19	20
Rowland.	40	34	17	1	11	4	1		15	19
Totals.	200	181	86	11	28	51	3	2		

disguised with the usual caps. The observer looked into the right mirror to see the reflection of the image as originally given in the left mirror. The second mirror gave a right-left as well as a front-back reversal of the real position of the fingers. The hands were so placed that but little could be seen of any primary images.

RESULTS.

1. The very large proportion of errors shows the strength of the illusion.

2. This amount is so great that there is little chance to mass errors in either hand. The three cases where there is such a tendency conform to the chief type in Table XI.

3. The predominance of wrong movements is in the symmetrically opposite finger and those fingers in the opposite hand that lie next to the indicated finger. These are exactly the places where one would expect the wrong movement to be made. Where a new adjustment is made for the front-back reversal, the observer knows where on his hand the finger lies that he would move, but he mistakes the hand. Where a new

adjustment is affected for neither reversal, the observer knows where in a given half of the hand the movement should be made, but he confuses both halves and hands.

TABLE XIII.

A. MIRROR FRONT. BACK OF LEFT HAND AGAINST PALM OF RIGHT.
CAPS ON ALL FINGERS.

	No. Exper.	Errors.	Following Mirror.			Next Finger Same.	Miscell. Same.	Right.	Left.
			Next Finger Opposite.	Symmet. Opposite.	Miscell. Opposite.				
Baldwin.	40	8	3			4	1	2	6
Emerson.	40	9	5	1		3			9
Kleinknecht.	40	17	9	1	1	6		10	7
Rouse.	40	12	3	2	4	3		5	7
Rowland.	40	21	15	2	3	1		1	20
Totals.	200	67	35	6	8	17	1		

B. AS ABOVE, EXCEPT REVERSED RELATION OF HANDS.

Baldwin.	40	9	3			4	2	5	4
Emerson.	40	14	12			2		2	12
Kleinknecht.	40	19	12	4		3		5	14
Rouse.	40	16	5	4	2	5		8	8
Rowland.	40	15	8	6		1		3	12
Totals.	200	73	40	14	2	15	2		

TABLE XIV.

A. MIRROR $\angle 20^\circ$ RIGHT.

	No. Exper.	Errors.	Following Mirror.			Next Finger Same.	Miscell. Same.	Right.	Left.
			Symmet. Opposite.	Next Finger Opposite.	Miscell. Opposite.				
Baldwin.	40	21	9	3		8	1	11	10
Emerson.	40	23	9	8		6		15	8
Kleinknecht.	40	22	18	2		2		17	5
Rouse.	40	37	34	1	1	1		19	18
Rowland.	40	23	14	5		4		5	18
Totals.	200	126	84	19	1	21	1		

B. MIRROR $\angle 20^\circ$ LEFT.

Baldwin.	40	24	20	3		1		10	14
Emerson.	40	21	18	1		2		12	9
Kleinknecht.	40	15	13			2		10	5
Rouse.	40	18	17			1		13	5
Rowland.	40	20	7	10	1	2		6	14
Totals.	200	98	75	14	1	8	1		

4. The difference in the amount of errors for the front-back and the right-left illusion indicates that adjustment to the former is much more easily effected. Our practical use of mirrors helps us to overcome the first illusion. The second sort of experience is relatively novel.

5. The number of errors that cannot be directly accounted for by the influence of the visual position is nearly negligible.

6. The right-left illusion is stronger when the left palm is up; the front-back illusion, when the right palm is up.

The conditions of the experiments in these tables were arranged to show the general principle of visual control, hitherto copiously illustrated, in yet further ways. For Table XIII., the hands are placed back against palm, the fingers interlocked, with the little fingers on the outside, and the line of the fingers parallel to the plane of the mirror set up in front. In the experiment of the other table the fingers were clasped palm up, the line parallel to the median plane of the body. The mirror was placed at an angle of about 20° with the median plane. This angle was made as small as possible consistent with a convenient view on the part of the observer. The arrangements in both these cases were to reverse in appearance the position of the hands with reference to each other.

It should be said of the first set of experiments that the position chosen was so difficult a one that it was nearly impossible to keep each set of fingers in lines parallel to each other and to the mirror. Such displacements tended to produce reversals among the fingers of a single hand. This probably accounts, in part at least, for the erroneous movements made with the correct hand, though these are certainly not in excess of similar errors in Table XIV., where such an explanation is not possible.

The caps were used in Table XIII. because the clasped fingers did not symmetrically correspond, and the resulting differentiation, if seen, might lessen the illusion. This reason did not hold in Table XIV.

RESULTS.

1. The expected illusion occurs in both cases.
2. The heaping of errors in Table XIV. on the finger sym-

metrically opposite, and in the other table upon those fingers of the opposite hand that lie next to the indicated finger, is due in both cases to the same cause, viz., their occurrence in parts of the opposite hand spatially corresponding to the indicated finger.

3. The noticeable tendency in Table XIV. to a movement of the next fingers, either on the same or the opposite side, confirms earlier results; and we have already seen (conclusion 5 under Table I.) that the error cannot be set down wholly to resemblance.

TABLE XV.

SUMMARY.

					Supination.								Pronation.			
	Table I.		Table II.		Table III.		Table V.		Table XIII.		Table XIV.		Table IV.		Table VI.	
	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.	Right.	Left.
Baldwin.	7	8			11	14	3	5	2	6	11	10	4	5	6	10
Emerson.	22	42	14	9	17	16	10	6	9	15	8		4	2	5	6
Kleinknecht.	8	11	7	9	1	1	8	8	10	7	17	5	1	2	6	2
Rouse.	6	58	5	5	15	19	5	16	5	7	19	18	4	4	7	19
Rowland.	9	31	2	14	16	10	2	10	1	20	5	18	1	10	4	5
Miller.	5	53			22	23										
Baldwin.									5	4	10	14				
Emerson.			6	13					2	12	12	9				
Kleinknecht.			3	14					5	14	10	5				
Rouse.			5	7					8	8	13	5				
Rowland.			5	13					3	12	6	14				

Table XV. presents a summary view of the failures to make the correct movement as these appear in the right and the left hands. All the tables are included where both hands agree in pronation or supination. This particular condition, as it is found in the unsymmetrical relation of the hands, has already been discussed in connection with Table XI. Here the results for Tables I., II., XIII. and XIV. fall into one group, as being concerned with an illusion that tended to throw the movement over to the opposite hand; while the remaining results were obtained where the illusion tended to divert the movement to the opposite side of the same hand. Tables III., V., XIII. and XIV. are concerned with positions of supination, and Tables IV. and VI. with pronation. For Tables I. and II. the position is a combination of both.

RESULTS.

1. The tendency to mass errors, where it occurs at all, shows a drift toward the left. Of twenty-six instances, twenty are of this type and six of the opposite type. Four of the latter are confined to one table (Table XIV.).

2. The existence of such a tendency seems to be connected with the illusion that throws the erroneous movement over to the opposite hand. Twenty-one instances occur in Tables I., II., XIII. and XIV., where the illusion is of this type. The remaining five are scattered through the other four tables.

TABLE XVI.

COMPILED FROM EXPERIMENTS EMBODIED IN TABLES VII. AND X.

		Erroneous Move.				Failures.			
		2	3	4	5	2	3	4	5
Baldwin.	Right.	4	1	21	5	12	13	2	6
	Left.	6	11	24	7	15	17	8	6
Emerson.	R.	2	4	21	10	15	17	5	2
	L.			24	13	15	18	1	1
Kleinknecht.	R.	10	10	6	1	4	1	12	13
	L.	9	15	2	7	10	4	10	10
Rouse.	R.	10	8	11	13	12	5	10	7
	L.	18	16	7	10	16	5	19	19
Rowland.	R.	15	17	4	2	5		12	10
	L.	21	6	3	3	10	4	14	16

The numbers at the heads of the columns indicate the fingers in order, beginning with the forefinger.

The interesting questions naturally occur whether there is any tendency (1) to make more erroneous movements with one finger than with another; and (2) whether more failures occur similarly. To answer these questions the eight hundred experiments of Tables VII. and X. were worked over to discover the distribution of errors and failures among the fingers. Table XVI. presents the details.

RESULTS.

1. The several observers do show a preference among the fingers in erroneous movements and also a massing of failures but they disagree with each other.

2. The right and left hands show a somewhat remarkable agreement in distribution for any one observer. Out of the

forty cases, there appear to be but five where the relative distribution in the two hands is markedly different.

TABLE XVII.

TO SHOW DRIFT OF ERRORS TOWARD THUMB OR LITTLE FINGER.

	Table III.		Table V.		Table VII.		Table VIII.		Table IX.		Table X.		Table XII.	
	I.	V.	I.	V.	I.	V.	I.	V.	I.	V.	I.	V.	I.	V.
Kleinknecht.	1	1	15	1	4	4	19	4	17	9	27	14	40	12
Rouse.	20	13	12	2	46	15	17	16	30	14	25	17	24	13
Rowland.	13	3	10	1	22	13	27	8	23	9	25	4	29	10
Totals.	34	17	37	4	72	32	63	28	70	32	77	35	93	35
Emerson.	12	17	5	11	10	16	1	34	4	17	5	32	16	22
Baldwin.	2	21	6	2	13	23	14	25	19	23	6	29	18	23
Totals.	14	38	11	13	23	39	15	59	23	40	11	61	34	45

Of the Roman numerals at the heads of the columns I. means thumb and V. little finger.

Table XVII. presents a new analysis of the results of the tables summarized therein. All the erroneous movements that were not made with the symmetrically opposite finger were classified on the principle of their occurrence either thumbward or toward the little finger from the indicated finger or its symmetrically opposite fellow. The observers were distributed so evenly between the two classes that they are separated in the table into two groups. Those of the former tables are included in this survey that showed the largest amount of errors falling elsewhere than on the finger symmetrically opposite.

RESULTS.

1. The observers fall into two opposing groups, each showing a very consistent tendency of its special type, and one a very large one.

2. Considering each observer separately, we find that in one case only is there a direct contradiction of type, while in but three cases is neutrality almost or quite complete.

In what direction are we to look for an explanation of the facts that have come forward in the course of these experiments? In the first place, there is the fact of the existence of an illusion connected with an abnormal position of the members. We

found this to be due in nearly every case to the abnormal visual factors, since their removal destroyed the illusion. A single observer in the Japanese illusion seemed to show that abnormal kinæsthetic factors were involved in producing it. We have to do here with a special case of neural habit. Visual cues and, more rarely, kinæsthetic cues have become in practice the well-defined guides of movement, to such an extent, indeed, that when these become untrustworthy through a change of conditions, it is only by effort, more or less, that the movement normally connected with them is prevented from occurring.

This principle seems to be illustrated yet further in our results. It appears that adjustments seeming equally easy to both hands in normal positions are less easy for the left than for the right when the positions are abnormal, as in our experiments, though the hands agree in position; and it appears further that for the supinated hand the adjustment is also more difficult. In other words, the neural habits underlying our practice in the control of our movements are primarily adjusted to a given space relationship of members; while plasticity is greater for the right hand than for the left, and for either hand pronated than supinated, though in the latter case we must not forget that for one observer just the reverse was true. The foregoing difference between the right and the left hands seems to be in line with the greater ease in control of the right that we find in many normal movements, though in the one we have investigated that difference had disappeared, yet only to reappear, as reversion to an earlier type, under the condition of abnormal position. This greater adjustability in one half of the brain than in the other half we can view as related to practice. A similar account is possibly justifiable for the better control of the pronated hand, though we have still to dispose of our consistent exception. One is tempted to formulate a hypothesis along the familiar lines of the 'sensory' and 'motor' types, thereby saving our main principle in this case. For example, let us make the following suppositions: (1) Less vivid sensations represent our limbs in consciousness when they are normally than when abnormally disposed. (2) In the character of the motor discharge either the nature of the incoming currents

or the situation of the centers may be prepotent. If the nature of the incoming current prevails, then the less familiar the situation the better the adjustment, and vice versa; but if the situation of the centers prevails, then the more familiar the outward situation, the more correct the response. The former is the 'sensory' type, in which must be classified the single observer whose control is best over the supinated hand; while the rest of the observers belong to the latter or 'motor' type.

The tendency of the erroneous movement to be drawn toward either the thumb or the little finger, according to the type, may be due to the more habitual employment of the members that lie on a given side. The difficulty with this view is that one would expect all erroneous movements to be drawn thumbward, since that side is probably in all but rare cases the stronger. Individual tendencies to favor or fail in a given finger have probably a share in the explanation accorded to the foregoing fact.

We have reason to believe that resemblance plays some part in the drift of erroneous movements toward the finger symmetrically opposite; but the amount of this error when the fingers are disguised with caps suggests the existence of an additional factor, perhaps purely physiological. In this direction points also the prominence of the fingers next to this and to the indicated finger in wrong movements; for we found in the discussion of Table I. that resemblance as a complete account of this case was out of the question. The precise nature of this additional factor is obscure to the writer.

There is further obscurity about the connection between the prevalence of failures in a given hand and the presence of an illusion that tends to throw the movement over to the opposite hand. The strength of the evidence for such a connection we saw in Table XV.

SUMMARY.

1. The influence of abnormal position upon the motor impulse, under the conditions of these experiments, is to change its direction in certain well-defined ways (cf. all tables).
2. There is a strong tendency to move the finger that really is where the indicated finger appears to be (cf. all tables).

3. That visual factors control the movement is shown by the disappearance of the illusion when touch is added to vision, or where vision is excluded and the stimulus is auditory. Its failure to disappear in the latter case for one observer shows that occasionally abnormal kinæsthetic factors can rise to importance (cf. discussion under Table I.).

4. There is a greater tendency to a wrong direction of the impulse if the indicated movement is to be made (1) with the left hand (Table XV.), and (2) with the supinated hand (Table XI.). A single observer out of eight is pretty consistently of the opposite type in (2).

5. This tendency to mass failures in a given hand is not due to the greater difficulty of recognizing as individuals the fingers of that hand. Cf. Tables VII.-X.

6. In the case of the Japanese Illusion, it is not due to a greater strain on one wrist than on the other. Cf. Table II.

7. The prevalence of failures in the right or the left hand seems to depend upon the conditions favoring that form of the illusion that throws the movement over to the other hand (Table XV.).

8. Individual observers are inclined to favor particular fingers in erroneous movements and to fail more frequently in control of one finger than of another; but among themselves the observers are very divergent (Table XVI.).

9. There are subordinate tendencies to move: (1) The fingers next to the indicated finger on the same hand; (2) the symmetrically opposite finger, and (3) the fingers next to the latter (cf. all tables).

10. The tendencies described in (1) and (3) above are not due to the resemblances between the correct and the wrong finger. An examination of the results in Table I. showed that the middle and ring fingers, which resemble each other most of all, were not mistaken for each other with more significant frequency than the thumb and forefinger.

11. There is a further tendency for wrong movements to be drawn toward the thumb side of the hand, in the case of three observers, and toward the little finger in the other two (Table XVII.).

12. The existence of the illusion is based on the law of neural habit. Our habitual dependence upon the visual cue in controlling our movements leads us astray when that cue no longer truly represents the actual situation. Failures are more frequent in the left hand because finer adjustments are less habitual to it. For that reason they are more frequent in the position of supination. The condition of the centers is prepotent in determining the reaction. In the exceptional type in which failures occur more frequently in pronation, the reaction may be viewed as determined chiefly by the incoming currents. Here the less familiar the situation, the more vivid the accompanying sensations and the better the adjustment. In the former, the more familiar the situation, the more correct the response. For the other facts in this summary, I can give no explanation.

The observers taking part in the work were students in the Harvard Psychological Laboratory, one being an instructor. Of the number, two were women and six were men. I acknowledge most heartily their coöperation, as well as that of Professor Münsterberg, to whom I owe the suggestion of the problem.¹

¹ The MSS. of this article was received April 14, 1904.—Ed.

DISCUSSION.

MIND AND BODY—THE DYNAMIC VIEW.

It requires a certain temerity to reopen the perennial problem presented by the apparent dualism of mind and body. It might appear that the last word worth saying had long since been said. It is, however, indisputable that the point of view of psychology, and, to some extent, of philosophy also, is changing. At least its language is changing and this change is distinctly favorable to a new statement, if not a solution of this problem. Accordingly, a number of valuable contributions to the literature of this subject have appeared within the last few months and the evidence that a monistic construction is desired by nearly all is cumulative. As Professor Moore says: " 'Life' experience is one inclusive activity of which consciousness and habit—the psychical and the physical—are, to the last analysis, constituent functions."¹

The present tendency on the part of the physical sciences to escape from the shackles of a material hypothesis offers a 'psychological moment' for philosophy to capture the entire forces of both combatants.

In advance attention must be called to the fact that there is no dualism in any one science, neither can there be. Biology has no body-soul controversy; neither has psychology, as such. It is only when we attempt at the same time to use both sets of criteria that dualism arises. The psychological subjective-objective dualism is a polarizing of what is and always must be a single activity into two aspects, it does not create a pair of incommensurables. It follows that this inquiry very naturally assumes the form indicated in the article entitled 'Mind and Body,' by J. Mark Baldwin.²

"The distinction between phenomena of mind and body, considered as distinct types of presented phenomenal change, requires the use of two distinct categories of construction, the genetic and the agenetic. Physical science it is which interprets the agenetic. Its explaining concept of cause is illustrated only and always in transformations of energy. On the other hand, is the special realm denomi-

¹ *Univ. Chicago Contrib. to Philos.*, Vol. III., 1.

² *Princeton Contributions to Psychology*, III., 2.

nated 'subjective.'" (The author adds that 'life processes are really genetic,' an admission which will greatly influence our attitude toward the distinction between genetic and agenetic as here defined.)

The problem is formally set in the following inquiry: 'Can we hold each set of phenomena to its own legitimate construction, and at the same time, reach a comprehensive conception of the concomitance of mind and body under which the scientific formulas appropriate to each may be given full value?' (*Ibid.*, p. 38.) This question becomes more pertinent if this author is correct (as we believe him to be) in saying 'that the present forms of the interaction theory involve a confusion of categories, due to the failure to maintain a consistent level of mental development.' (*Ibid.*, p. 39.)

"Philosophy asks: How can we think reality in one thought? In terms of our present discussion, how can body and mind, being what we have come to think them to be, live hospitably housed together in one phenomenal group of facts?" These questions are such as to arrest our fullest attention and awaken our keenest interest. This statement of the problem is most helpful and necessary to further progress, but the answer given in this place is tentative and exploratory. That a single and simple solution is ultimately expected is indicated by the italicised phrase: 'All this means that the world is, after all, one and that the categories of mental construction, derived in a process of evolution by actual treatment of the world, *cannot finally reflect processes in essential contradiction with each other.*'

This is, in fact, the criterion of congruousness, which is the last appeal and unanswerable argument of monism. The universe is an organism and contradictory categories could not have developed under a law of evolution. It is quite disappointing, therefore, especially after an appeal to an '*all-comprehensive and completely full experience*' as the content of 'æsthonomic idealism' to learn that 'psychological parallelism then is, from the point of view of science, our positive catch,' even though there is 'hope for a theory of correlation of these characters which will yield a higher adaptation in the whole realm of science.' This is the more disappointing in that the one-sided and unsatisfactory nature of a simple scientific solution has just been insisted on. But Professor Baldwin modestly refuses to expose to view the statement of the metaphysical solution designated as *Æsthonomic Idealism* and we are left with one foot on biological foundations and the other on psychological conclusions but with the door of hope open before us. It was inevitable that others should take advantage of this fresh statement of the problem to attempt this next

step which is to land us with both feet upon some monistic construction.

It is, at any rate, certain that the correlation sought cannot be in either of the partial realms. Neither biology nor psychology, as such, can hope to afford a solution which involves both of them. The unity must be sought in a field large enough to include both.

Nevertheless, it is important for our purpose that we should get the formulated results of both to be carried up into the higher sphere. In order to secure this material a brief survey of these contiguous fields will be necessary. It must be noted in advance that the net result in each of these cases is of one kind; there are no incommensurables or incompatibles in either sphere. These appear only when the ultimate data of biology on the one hand, and psychology on the other, are attempted to be compared (and this attempt is made in terms of one or the other of these sciences) that incompatibility appears. The suggestion is obvious that the incompatibility arises from the methods and not from the content — or, in other words, from the impossibility of attempting psychological structures with biological tools, and *vice versa*.

We may also anticipate our conclusion in so far as to call attention to the way in which the problem set for us by Professor Baldwin is disposed of by the so-called 'functional school' of psychologists who save us the trouble of further discussion by denying the existence of any problem. But it is notorious that, a quarrel once on, it is a work of supererogation to show that there is nothing to quarrel about. It is when the quarrel is over that the proof of its futility is balm to our wounds.

The most concise and intelligible statement of this functional solution which the writer now recalls is that given by Professor Bawden in *THE PHILOSOPHICAL REVIEW*, XIII., 3, May, 1903. "Mind, as here viewed, is the totality of the functioning of matter (in so far as function may be said to imply end or purpose). The psychical is the *meaning* of the physical." "Mind is simply a collective idea for all the psychic functions of an organism — and the psychic functions are coextensive with the growth of an organism. Mind is not an entity behind the process of consciousness, it is that process itself. Mind is just as truly a growth as any other living thing." "It can be a growth only if of the nature of a process. Mental life is a continual synthetic construction. It is simply a name for the orderly continuous functioning of an organism under conditions of tension in adaptation" (p. 308).

Professor Bawden uses for the theory thus stated the title 'Functional Theory of Parallelism,' to which the present writer objects on

several grounds, two of which may be mentioned. First, there is an implied recognition of a material substrate — of a something of which the mental activity is a 'function.' Second, the theory is not one of parallelism except as one returns to the artificial dualism of isolated sciences. Or, to make the criticism general, the view point is that of psychology while the subject is germane to metaphysics. That this writer has himself recognized and pointed out the remedy for these supposed defects may be gathered from his article in Vol. I., No. 3, of *The Journal of Philosophy, Psychology and Scientific Methods*. "Under the name of energy, motion is now regarded as itself the essence of reality, and the idea of brute, lump matter drops away. In place of a static we get a dynamic theory of the nature of reality" (p. 63). Professor Bawden also points out the paradox insisted on by Professor Baldwin. "The solution of this apparent paradox lies in seeing that consciousness, taken apart from the organism which is conscious, is not an entity or thing or even a process; it is simply a meaning or significance. * * * After abstracting the psychical by definition, from the physical, there still cling to our psychological statements of the nature of consciousness traces of our conceptions of material objects. * * * Any thinking or speaking is a polarizing into two aspects in thought of what is an undivided unity for action. This, of course, is a methodological not an ontological dualism; hence, it is paradoxical only for him who forgets its methodological origin."

But these are passages by the way, and we may return to our own survey. As we have already seen, the difficulties in the historic attempts are due, in a very large part, to the attempt to combine in one discussion the methods and data of two or more diverse methods of investigation. Usually the biologist, who essays to discuss the relation of mind and body, is unable to complete his analysis as a biologist simply; he cannot forget that he is also a person, with experiences of his own which he feels sure are also repeated in the lives of the objective units he is discussing. He cannot divorce his *biological* discussion from its *psychological* interpretation.

This is, of course, implied in the very nature of the topic, for any discussion of the relation of mind and body implies the use of the tools or methods, as well as the data of two sciences, and the question at issue is just the inquiry whether these data are commensurable and whether these methods and tools can be employed in the same discussion. As a biologist I cannot consistently inquire as to the relations between mind and body nor can I, as psychologist, properly discuss the body, except as an image presented to sense. The question

reduces to this: Is it possible for the sciences of subjective and objective phenomena, respectively, to present to philosophy the results or interpretations of their research in common terms so that the unification (the real business of philosophy) can be completed.

First as to *biology*. One of its results is the recognition of living individuals. This is no easy matter nor can the discrimination be considered complete. Colonies and social groups imply lateral connection which appears in various forms throughout the series and the existence of which we must suspect in cases which by their nature prevent us from definitely recognizing it. Individual men are such units and biology busies itself in recording the complicated synthesis and coördinations of energy displayed therein. Reciprocal communication between part and part, mutual reaction of function upon function demonstrates a 'vital' relation of unity. No new force is discovered and, of course, no other than a physical force could be recognized if many existed. This may be claimed as matter of definition, for any phenomenon recognized by physical science would be *ipse facto* physical.

But there has been talk of a vital force. Such a term could only be a name for a coördination or a bond. Such a relation is a truth—a truth of the highest importance, and may well be worthy of a distinct name—but it is not a fact of the same order as heat, light or weight.

The recognition of a living unit is a fact of the same kind as the formation of the judgment of 'substance' or 'object.' 'A living object' is such a constant group of coördinated experiences as not only persists in established relations but proves adaptable to changes in the environment by reactions thereto without destroying the essential coherence of these experiences. A living thing is a construct similar to any other thing. One would not say that the inanimate object was created by cohesion, though that may be a name for a part of the observed coherence of attributes. Neither shall we gain by saying that the animate body is created or maintained by a vital force. Any given object, *e. g.*, any given man has his own individual formula descriptive of the totality of the reactions (or shall we say the trajectory or career). Not that we could express this formula by any means but such a formula could be conceived as possible.

Now our investigation of the individual man results in our determining certain partial elements in this all-inclusive formula. We get a little idea of the energetic phases resulting in circulation, respiration, innervation, etc. Sometimes we are fortunate enough to be able to

subsume several minor formulæ under one more general or more inclusive. We never doubt that the possibility exists of a synthesis which would show all these coördinated in one career. Of course it is soon discovered that many individuals are wrapped up in any one subject and that units of a higher order (species, etc.) can be formed —unities which are formulæ for a vastly more complex coördination yet presenting themselves to us in such wise that we are often able to approximate nearer to a total formula or statement of the career than is possible in case of the individual.

Now *as biologists* we observe the acts of the free individual and discover fundamentally no difference in kind between the secretion of bile, the peristalsis of the digestive organs and the most complicated free motions of prehension, locomotion, etc. There is biologically no difference between the act of the phagocytes preying on bacteria in the tissues and the Indian hunter in pursuit of bear and the Wall-Street broker preying on simple-minded citizens — each of these acts is beautifully adaptive. So far as we know, the image on the retina is as real an 'occasion' for the prehensile phenomenon that follows as the carbondioxide stimulus on the respiratory center is of the respiratory spasms which result.

We can biologically observe that the liver secretes bile; we can equally observe that action in the vicinity of the fissure of Rolando is followed by adaptive motions in the muscles of the limbs and that a stimulus in Broca's region is followed by reaction of the vocal organs. But it would be entirely incompetent for the biologist to say that brain action produces thought. Adaptive reaction is no proof of mentality as usually understood.

However, we are all born psychologists and, even though we deny the soft impeachment, we cannot escape this congenital peculiarity. We feel and sometimes we fancy that we think. We may now-a-days be a little afraid to admit volition but we still feel quite sure that other people are responsible for at least part of their actions.

These same physical phenomena, reported to our biological observation in terms of visual, tactual, auditory, and other reactions, are reported by the subject in terms of something which he alone can possess, viz., a subjective reaction, let us say a pain. But let us suppose that the subject of our study is also a trained observer. He might report to us as biologists the conditions of his own body as observed by him, that is, as he feels it, sees it, hears its vital movements, etc., and this information, if reliable, would become a part of our biological formula just as it would if we ourselves or some inde-

pendent observer had recorded it. In addition, this subject might report data which we could by no means know anything about, *e. g.*, a pain, or peculiar sensation, and he might locate it with reference to the previous data. This is also valid biological material — this information is so important that frequently a surgeon will not hesitate in bringing a life into jeopardy by an operation upon such testimony alone. He, at least, has no doubt that that particular sense of tenderness and pain indicates a modification of the normal biological processes in, let us say, the appendix vermiformis. But he does not make the mistake of trying to excise the pain — he is a consistent biologist and to him the pain is diagnostic simply. Even the so-called empiricists in medicine do not commit that mistake (except verbally). That is the pet sin of current psychology alone. To the biologist the reported pain is as objective a phenomenon as the tympanic reaction to palpation or the cessation of peristalsis.

The reported 'mental' reactions of a higher type, with all the adaptive interrelations, fit into his formula for the life so long as they are descriptive data only. From his own experience (as psychologist) he may clothe these reports in a garment of reality, for he has felt the like, but, as a biologist, they are just other forms of reaction, like the contraction of a muscle. The experience of joy or a minor pleasure is connected with circulatory, muscular and nervous activities, and one is a fact to be catalogued like the others. So it appears that the whole field of descriptive physiological psychology is a purely biological science and is to be cultivated with the same tools as any other department of biology. A great deal of unrealized hope and of futile effort might, perhaps, have been saved by an adequate realization of this classification. Whatsoever a man (biologist) soweth, that shall he also reap.

But meanwhile we must give the psychic its due. None of these biological achievements would have been possible but for the subjective reaction which has not only made it possible to perceive and to assemble data, but on the accuracy and adequacy of whose forms the possibility of all classification depends. It is not merely that the objective world reveals itself to us, but we have created this objective world in accordance with forms inherent in our subjectivity. It is not merely that our personal experience has stamped each elementary reaction with the certificate of reality without which it would be valueless, but the very form of the apprehension of the external world has been the product of the form of our subjectivity.

It appears, therefore, that so long as we persistently abstracted the

content of experience and the organization of it from the act of receiving and organizing the matter seemed simple, but when we ask ourselves, as sometimes we must, how it happens that we react as we do to the external world and not equally and indifferently otherwise, the difficulties of the problem appear.

Psychology may now examine the problem and attempt a solution from its own point of view. We now have to do with experiences as avowedly *ours*, *i. e.*, immediate realities. We have a multitude of presentations differing in *mode*. This difference we can never understand, we can only feel it. No Weber's law or periodic formula will explain why we feel light, taste, pain, etc. These are the data out of which all that we know is to be formed. There is nothing else. But a succession of different modes would never give us the contrasting perception of difference vs. identity on which all our psychological development rests. Here the old psychology demands its own, claiming that such recognition of difference (to put it simply) between presentations of sense in sequence implies a *tertium quid*—a soul—in which the comparison must be made. Just as, it is claimed, we cannot determine whether one figure is identical with another until it is measured by or in a third thing, so we cannot detect difference until the two compared elements are brought mutually into relations to another.

To this it may be replied that the ultimate test in geometry is *superposition*. In last analysis the demonstrations reduce to applications of this law of superposition. This analogy, if of any value, tends rather to the other conclusion that the perception of difference arises from the reaction between two presentations (or their several energetic grounds) superposed in such wise that the overlapping or non-agreeing part forms a new percept. Yet here too we imply a continuum. It is not a conscious continuum. There must be a somewhat persisting through a greater or less span of time which not only somehow preserves some counterpart of one impression, but receives a new one in such wise that the new one is different from what it would have been but for its predecessor. Things are going on that are not reported in consciousness—things which determine the mode of consciousness at this moment, and which preserve the effects of the energy involved in some preceding form of consciousness.

We have the curious anomaly then of living in a sphere (psychic) the grounds of which are indubitably in something else. This something else has been called the soul. The little rivulet of consciousness on the wave of which rides present experience is all that is open to examination. We strive to ascertain whether relations (cause and

effect, shall we say) can be discovered between elements in this wave of consciousness and others in other portions of the stream. But how do we now know anything even of the existence of these other events? Evidently the ground of their reproduction lies in the structure (*i. e.*, activities) of this *tertium quid* or soul. It appears entirely incorrect to speak of relations between successive acts of consciousness—the relations are between the total acts of which consciousness is one of the ‘meanings’ or modes. There is then no such thing, strictly speaking, as association of ideas. Is consciousness then but a feeble reflection of an inaccessible light and are such relations as we discover between successive flickers of the reflection dependent for their explanation on the reactions of the hidden light? Something like this, apparently.

This deeper light may be studied only through these imperfect, intermittent, one-sided, reflections—how imperfect only the trained psychologist can fully appreciate. And yet (lest we forget) these flickering reflections constitute our psychic life, *fide* current definitions. To say that they can by any means directly influence our inner light is absurd. No more could we kill our enemy by stabbing his shadow or feed our friend by offerings before his statue. Yet undoubtedly objective events do affect the psychic manifestations. This process might be illustrated by the actor who shoots the apple from the head of his unseen assistant by aiming with aid of a mirror, or by the Japanese fleet securing accurate aim at Port Arthur by wireless messages from vessels at a different angle.

We do not seek to communicate directly with our friend’s thought but we strive to send our message through eye or ear to that somewhat from whence the thought arises. Here is undoubtedly a formal expression of some sort of parallelism but it can hardly be called a psycho-physical parallelism. Physically we did not find any reason for assuming anything psychic at all. Why should we say that this psychogenetic somewhat is physical?

But perhaps it is not wholly clear that the conscious process does not react on the body. Let us look at it in another way. I feel fear and because I feel fear I react in a certain way. Not at all. This statement is contradictory to all that we know of animal activity. I feel fear because certain activities are coördinated in a peculiar manner, or rather, certain coördinations or equilibrated forms having been induced, I feel fear. Fear may be but one of the expressions of that coördination, and there are others, some of which issue in running away, screaming, etc. Fear is the reflection, shall we say, of a con-

flagration having many phases? The fact that I feel fear is not the 'cause' of my running away.

I communicate the occasion for my fear, 'a burglar,' to my neighbor. Did I communicate my fear to him? Not in the least. Neither did I communicate running away to him. The great wave dashes upon a rock and passes onward in a hundred eddies, but the sound that is produced at the same time did not produce the eddies. (Let us not push this figure too far.)

Psychology may construct a geometry for the relations between the various experiences and rest content that the expression corresponds to valid relations existing in the unknown ground of consciousness. But these elementary experiences are only immediate data—our only way of knowing this 'ground'—the rest are only formulæ for arranging them. Judgment is such a formulating activity but is not it determined by something inhering in the same ground? Is there any external reason why we should formulate the concept 'substance,' for instance, or does such formulation express but a phase of the constitution of the 'ground'? It would appear that the mechanism for testing truth as much as that in which 'reality' inheres, is something back of consciousness or of which consciousness is only one expression. The form in which my judgments are cast is a fact to be dealt with as much as the existence of mode itself, and each act of comparison or identification has a certain mode or feeling tone which stamps it as 'ours' rather than another's, and thus adds 'reality' feeling to the fact of thought though it in no way vouches for the 'truth' of its content.

It becomes apparent then that both biology and psychology become conscious of limitations and so are aware that there are facts outside of their boundaries which are nevertheless necessary to the full understanding of the living individual. Biology assembles observations of the behavior of the individual. No one observer is able completely to observe and so part of the information is reported by others and among the others there may be even the subject of observation himself.

The facts assembled by his own effort and that of his fellow laborers and even, to a certain point, by the observed individual are of the same kind, but the last mentioned is able also to report phenomena inaccessible to the others, yet these unique data fall into congruous relations with the others and supplement or confirm data of the direct or objective sort. Their validity it is foolish to deny and they become part of the biologist's material (pain, animal behavior, etc.).

The method of securing this information does not trouble the biologist who remembers that all of his data without exception were derived by inference from psychic acts or modes of experience. Psychic and physiological data come to us over the same route. It is when we seek to interpret these that we find it necessary to resort to a most complicated contrivance in our own mental activities for outward projection in one case and inward reference in the other. We are informed by the genetic psychologists that there is a stage prior to this polarization of experience in the development of the individual. If this be so we have really encountered nothing so far justifying us in setting up so fundamental a distinction as that between mind and body. The most we can say is that we discover in ourselves a difference between simple psychic acts (*i. e.*, immediate experiences) and the arrangements, relations, and inferences we are forced to make of them apparently as a result of some orderly or organic mechanism underlying or including the power to experience. Two things remain unknown and unknowable from the standpoint of both biology and psychology, viz., the reason for the modes of simple experience and for the forms of judgment based on them.

The problem is now appealed by both parties to a higher court. The trouble has been lack of jurisdiction in each case. It cannot be said that either department has found justification for separating body and soul. Each has recognized its limitations and, at first blush has been inclined to lay all the blame for the 'other' it discovers or postulates upon the rival science.

The trouble all along has been that the judge is also *particeps criminis* and the biologist can no more divest himself of psychological infirmities than the psychologist can forget that he is also human and so biological.

Metaphysics is therefore called upon to reconcile the residual and unassimilated results of both. Biology asserts that its field is a unit and everything harmonious so long as it does not consider the source of its information, but the moment that question is raised, it is forced to admit that all it has in the way of data is a mass of inferences or judgments the form or validity of which it can in no wise explain, and that these judgments are based on immediate experience in various modes, the differences between which are as unexplained as is the nature of consciousness itself. Biology therefore relinquishes this problem to psychology with some asperity to make of as much as possible. (It may be confessed that it is not very much that is made of it.)

Psychology catalogues experiences and names the forms of judgments and diagrams the observed relations, polarizing them into subjective and objective without finding any inherent difference between them and discovers that there is no direct relation between one experience and the next. As one feeling does not cause another there must be some kind of organic nexus behind experience. One thought does not call up another any more than the secretion of bile to-day produces a similar act to-morrow, both sets of phenomena are 'explained' as related to some organism or continuum. Psychology is prone to suspect biology and to think that a brain is the thing back of thought in which all psychological manifestations are bound together. When convinced of the futility of this suggestion it gives up the quest, simply concluding that the bodily phenomena are 'parallel' to the mental. This is nothing but a polite way of confessing defeat, or of keeping out of the quarrel.

One common element may be recognized in the midst of the obscurity of this discussion, viz., *forms* of activity. It is not the *fact* of energy but its *mode* that presents to science its multifarious material.

So when asked to arbitrate this dispute metaphysics offers some such result as is briefly given in the sequel.

But first a word as to the nature of energy. Of energy, in the nature of the case, nothing can be known except as expressed in the form of activity. Nothing is to be gained, therefore, by postulating matter or other entity, different from or behind activity, as a *cause* or ground of activity. As stated above, to us energy is known and *can only be known by its form or mode*. Behavior is the thing. Energy is the term representing the fact (all facts known or possible) concerning behavior. Dynamic realism definitively abandons the search for the unknown *ground* of behavior and claims that for any human philosophy the activity itself is the ultimate. It especially declines to be deceived by any analogy requiring us to know what by nature and definition must ever remain unknown, viz., matter, a something itself incapable of action, but the ground of all action.

But energetic form may be viewed in two ways. Otherwise expressed, all activity in a world of reaction expresses itself in two classes of modes, one which we may call intrinsic, the other extrinsic. This is a direct result of a law, which is clear enough from the physical side but has hardly been sufficiently appreciated in philosophy; namely, that activity is meaningless without resistance. Any expression of energy *in a universe* is dual in its manifestation. We could perhaps imagine, or at least, speak about unimpeded energy or 'pure

spontaneity,' which would possess only an intrinsic mode. Its meaning would be for itself alone. No such *manifestation* of energy is possible. Physically, action and reaction are constantly associated and equal. A single or isolated force is impossible. In metaphysics, reality is the reaction of objective and subjective — the 'affirmation of attribute.' Morally, the solution of the problem of good and evil, from this point of view, is that the real good is a doing or striving, and the evil is the condition of such strife; this is good in the making but evil if unvanquished. (See Paulsen's System of Ethics.) Metaphysically speaking, every being in every phase of its career has a double meaning — a meaning for itself and a meaning for the universe. Illustrations are apt to be misleading or unconvincing, but let us use a psycho-geometrical analogy. We may suppose that a certain type of being is represented by an elliptical orbit or trajectory. This activity will impress itself upon adjacent (in Lotze's sense) energetic modes and the form, extent, and result of this activity will depend on the nature or mode of the activity in question (here represented by an elliptical trajectory). The resulting readjustment may be supposed to extend indefinitely. The universe as a whole is different from what it would have been but for this particular energetic manifestation. This is the extrinsic side. Now this being is known to the observer, not by what it is, but by its extrinsic effects, by the impress it makes on the universe, or, more particularly, on the immediate environment of the observer.

But there is another way in which our ellipse must be viewed. As a result of its activity upon the world, the world has reacted upon it. The trajectory is thereafter a different kind of ellipse for having reacted with the rest of the universe. Its intrinsic nature has altered. Its locus formula would have to be rewritten. The inner meaning is constantly changing. The next time a reaction takes place the effect will be different from that of the former activity.

Now suppose, as we must, that certain sorts of trajectories or modes (not to say all of them) express this intrinsic form in terms analogous to consciousness. This psychic mode is the intrinsic meaning corresponding to the given locus formula.

A still further suggestion could be hazarded: It might be supposed that a certain degree of complexity would be necessary in order to reach any particular type of conscious expression. Then, if there were complicated systems of equilibrated energy (say human bodies) which were subject to cyclical or rhythmical variations, it is possible for the equilibrated unit to drop from a state of extreme complexity,

with an intrinsic mode of consciousness, into one not intrinsically capable of consciousness in any given form. Later on, in another phase, the activity could again rise above the 'dead-line' into that phase whose intrinsic form is psychic. In the interval below the 'dead-line' we say the subject sleeps. What the 'genetic modes' of the equilibrated unit might be no one can tell till he himself experiences them.¹

But how does it happen that we feel our conscious life as a continuum? So far as our feeling it is concerned the question does not need to be asked, for we have no mechanism for recognizing the hiatus, but there is that behind which bridges the hiatus yet to be accounted for. It might be said that the intrinsic form varies sympathetically in response to every influence and retains such segments of past experience as serve to connect all in a present unity of experience.

The ground for our confidence in the general correctness of the data of mind is to be found, especially from the evolutionary point of view, in the belief that all these forms of energy have been evolved by interaction and that the influence of one part is justly and adequately expressed in every other part. This is what we mean in metaphysics by describing the universe as an organism. On this basis alone a monistic interpretation is possible.

The view just expressed cannot be called parallelistic except by doing violence to the usual form of statement of parallelism and,

¹ Perhaps the most apt physical illustration of the idea of psychical equilibrium advocated by the present writer may be gained by the study of the gyroscope. I am not aware that the mystery of what Foucault called the 'fixity of the plane of rotation' and what Tait and Thomson describe as 'gyroscopic domination' has ever been adequately explained but we may easily convince ourselves that composite motions of revolution may be so adjusted as to acquire a high degree of independence of external influences (such as gravitation) and to present great resistance to impacts from without. Such a system becomes gyrocentric.

The formula given for the estimation of the angular velocity, etc., of the gyroscope is sufficiently complex and we can only faintly imagine the difficulties in the way of constructing a formula covering all phases of gyroscopic interaction—of wheels within wheels. But when one contemplates the complexities which must characterize the gyrocentric activities coöperating to produce the type of equilibrium required to produce a thought imagination is quite at fault.

There can be no doubt that the concentric equilibrium produced is capable of offering a very high resistance to external impacts in some directions while being, like the gyroscope, exceedingly sensitive in its responses to influences in other directions. In other words, the nature of the response is directly a function of the form of the equilibrated forces.

similarly, it can be classed with 'identity' systems only at considerable hazard of misconception. We prefer to speak of it simply as dynamic.

In details it is very hard to present this view in such a way as to give to it the same pleasing objectivity which accompanies the idea of a material brain grinding out thought as a mill grinds out flour. If we admit that the complicated equilibrated organism of our being developed under the law of evolution it need not surprise us that the reaction corresponding to sensation of redness is an invariable counterpart of some particular orderly happening in what we call the objective world, nor yet need we consider it impossible that, under the same law, that peculiar conscious reaction which we call a judgment of 'substance' (always some particular substance) corresponds with coördination having a constant value as representing an objective thing. So on indefinitely. The most complicated coördinations of our mental life have a meaning which expresses a real (evolutionary) correspondence with other things in the universe (objective realities not otherwise known to us). Even the much discussed concept of 'freedom' must have its value — it is somehow true. However much its philosophical interpretation may trouble us, if we are consistent evolutionists and fully grasp the meaning of the word 'dynamic,' we must accept its practical implications as genuine.¹

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